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The social construction of science: a theoretical and empirical investigation of aspects of the institutionalisation of the physical sciences

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THE SOCIAL CONSTRUCTION OF SCIENCE:

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DECLARATION

This thesis is an original piece of research, the main content of which has not been previously submitted for a University Degree or other similar Award.

Tom Jagtenberg

Dedicated to
my Mother and Father.

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To all my friends - Julia, Ian, Stephen, Robin and others, I thank you for your patience and indulgence. To Becky, words are not necessary.

There is at least one problem in which all thinking men are interested. It is the problem of cosmology; the problem of understanding the world including ourselves, and our knowledge, as part of the world. All science is cosmology, I believe . . .

Karl Popper, The Logic of Scientific Discovery, New York: Basic Books, 1959, p.15.

ABSTRACT

This thesis concerns the institutionalisation of the physical sciences.

The thesis breaks with the established traditions in the history, philosophy and sociology of sciences by attempting to capture both the cognitive and social dimensions of institutionalisation in one unified analysis. This unification has been achieved through a treatment of research as goal directed social action. This theme has been developed both theoretically and empirically.

Theoretically, the thesis draws on a range of sources but the main inspiration has been the phenomenologically inspired work of Alfred Schutz - particularly as explicated by Thomas Luckmann and Peter Berger. The theory that has been developed has been supported by two case studies of Australian researchers - a group of physicists involved with the inspiration and development of an economically viable thermally based solar energy array, and a group of neuro-pharmacologists investigating aspects of chemical transmission systems in the human brain and their relationship to schizophrenia.

The case material involves aspects of both structure and process in the life worlds of physical scientists. These aspects have been explored in considerable detail through the development of a system of eighteen related hypotheses. The overall picture of the physical sciences that is presented though, is one of structured sub-universes of meaning constituted through the actions of professionalised scientific workers.

Scientific research is portrayed as a highly social process with researchers working together as part of research programs. Research programs are defined as the primary locus of productive activity and are constituted through the (typically) collective activities of a group of research workers who share a commitment to particular research practices and techniques, who are directed in their research towards a shared set of goals, and who share, to some extent, a common stock of specialised knowledge.

The sub-universe of the research program is not without its conflicts and discontinuities, however. Researchers were observed to alternate between contexts of research and contexts of legitimation, which in the case of the solar energy researchers was a highly institutionalised separation of structures of relevance. This movement between contexts was in some cases associated with the experience of conflict in which scientists at times, found themselves in double bind situations where the demands of a more inwardly directed professionalism competed with demands of social relevance.

The scientific research described in the case studies was predominantly instrumental by virtue of being more highly directed towards technical goals and the means for their realisation than towards questions about the value of these goals. It is suggested that this instrumentalism is typical of all of the contemporary physical sciences.

The field work conducted in the course of the thesis involved the innovation of a method of "repeated feedback". In this method research accounts were generated through an iterative process which relied on the scientists to check and up-date a series of descriptions of their research. These descriptions were based on open ended interviews, questionnaire responses and non-formal interaction. Insofar as the method can be used to prevent unintended discrepancies between a sociologist's impressions of scientists' research and scientists' understandings, the method is particularly useful for the generation of accurate research accounts.

CONTENTS

LIST OF TABLES AND FIGURES

CHAPTER 1: INTRODUCTION

This thesis is a sociological investigation of the natural sciences. In more precise terms, the thesis deals with the *institutionalisation* of scientific research - that is to say, with the *patterns* of meaning and action which constitute science. The basic assumption behind this analysis of institutionalisation is that scientific research has a social dimension which extends through the actions *and* consciousness of scientists.

This general assumption is consistent with what one could call a "sociological perspective". In the sociology of science the assumption has not, however, been the subject of the theoretical and empirical scrutiny it deserves and as a consequence the nature and potential of the specialty has tended to remain unclear. This thesis has developed out of my interest in this and related basic assumptions of theory and research in the sociology of science and allied specialty areas such as the history and philosophy of science.

Whilst this thesis is generally concerned with a number of different aspects of the social nature of science, the analysis does have a particular focus - research in this thesis has been partially directed through the specific channel of an investigation of the goal directed nature of scientific research. This particular focus has developed out of an awareness that the existing literature in the sociology of science rarely concerns itself in any practical way with what the goals of scientists are, how they effect research and the extent to which scientists' goals are social products and expressive of social interests. At the very least this means that the sociology of science tends to remain ignorant of an important aspect of the nature of scientific action and the mode of its institutionalisation; at most, the general field of science studies can be said to lack an understanding of the goal directed nature of science. In an era where the planning of science has become important for ecological, political, economic and social reasons such ignorance, particularly at the sociological level, is inexcusable. This thesis has been developed then, with the overall goal of arriving at a deeper understanding of the social nature of scientific research.

At root, the problem of shallow conceptualisation in the sociology of science resolves into a variety of images of science that have been held by many scholars including sociologists, philosophers and historians of science. These images all presuppose that the natural sciences are somehow influenced by social forces, but the implications for individual scientific action remain generalised and, in all likelihood, inaccurate. Ravetz [1973] has summarised the position in a revealing way. As an expression of an awareness that science is neither invariant nor homogenous, Ravetz has argued that three distinct varieties of science have arisen historically and have been associated with particular interest groups:

i) "Pure, academic science" which was based on an ideology derived from nineteenth century German universities:

> Here science is totally inward looking, its only offerings to the outside world are general contributions to knowledge and culture, unpredictable technological applications, and the example of its endeavour". [Ibid:211].

ii) "Ideologically engaged science", which is considered a bearer of truth and reason, standing against dogma, superstition and oppression, and a weapon in the struggle against a variety of material and spiritual ills:

"By its very nature science could not produce either error or evil, and so it had a privileged position among all sorts of ideologically engaged activities". [Ibid:211].

iii) "Useful Science" where "the results and methods of science are applied directly to technical and practical problems; and those external tasks provide stimuli, goals and partial justification for scientific work". [Ibid:212].

These images are useful generalisations about the nature of contemporary science, but they remain sociologically problematic - we are not told about the distribution of these types in modern society, nor are we informed of the bases of these types in specific individual action. The consequences of such sociological naivete which, as remarked earlier, characterises science studies in general will become apparent in a subsequent chapter where it will be shown that the images that scientists and their managers project may not be complete or accurate. Certainly, though, *appearances* seem to indicate that the last type of science is becoming increasingly appropriate as a description of contemporary science. Research has become concentrated in industrial and government laboratories, [1] where the demands for profit and growth and accountability, respectively, require that the research be directed, at least in the long term and more often in the short term, to practical ends. Furthermore, research appears to be directed primarily towards the general objectives of economic development and national security. [2] Or at least this is what the large, state funded surveys tell us. In Australia, for example, analysis of the results of "Project Score", the only systematic nationwide survey of research in the sciences, demonstrates that less than 10% of research could be discussed in isolation from identifiable socio-economic categories - such as the broad objectives of economic development, business, national security, etc. [Hill and Jagtenberg, 1977:12].

In this thesis I intend to challenge this observation that science is becoming increasingly directed towards socio-economic ends. The first question we must ask ourselves is "what kind of information is necessary before we could possibly reach such a conclusion?" Clearly, any information merely based on scientists' assessments of their own research has to be regarded with considerable suspicion. Even if scientists were not deliberately intent on presenting images most consistent with their interests and security we would still need some way of checking scientists' accounts, since it is quite normal for anyone to be unaware or mistaken about the way that others actually categorise their work. It is even more likely however, that any research is directed towards a number of objectives of differing levels of generality and that consequently it may not be possible to categorise research uniquely. In other words, more information about the "reality" of scientific research is required before we can make any confident generalisations about the socio-economic orientations of modern science or, indeed, about the general relationship between science and society. This means, amongst other things, that mass survey data, such as that provided by the OECD [3] and Project Score, should be regarded as only one level of information. Useful as mass surveys may be for the indication of trends in easily quantifiable phenomena they cannot, under the normal circumstances of their administration, provide reliable information about more complex subjectively mediated phenomenon. It may be, for example, that scientists who profess to be highly concerned and constrained by social, economic and political factors are still in their research relatively uneffected by the pressures of social relevance. The responses a scientist will make to a relatively anonymous questionnaire may be little more than an indication of what the "official" picture ought to be to best protect the scientist's livelihood.

But what kind of information do we need to test the generalised assertions of mass surveys? Some method which enables the comparison of what scientists say and what they actually do is indicated. Difficult as that task may actually be, there are techniques available in the social scientific repertoire which have evolved with precisely that task in view, and which have, furthermore, developed in popularity as a consequence of the failings of mass surveys to provide any more than fairly trivial information about the life world of individuals. For example, the techniques of participant observation and non-directive interviewing enable the social scientist to penetrate a little further into the complexities of human motivation, intention and practice than does the routinised administration of questionnaires.

In this thesis I have developed methodological and theoretical tools that can provide "in depth" information about the thoughts and actions of scientists. Since this goal obviously opens up a vast field of possibilities, I have naturally been selective about the kind of information actually generated. In the theory and two case studies which follow, I have concentrated on the development of "maps" which cover the beliefs, goals, theory and techniques shared by individual scientists working together as a team. These maps of various structures that scientists share have been developed in the context of an analysis of a variety of processes through which theoretical and material products were generated by the scientists being observed. On this basis it has been possible to make a number of generalisations about structures and processes in the research "worlds" of two small groups of university scientists.

The scope of a thesis concerned with general questions about the institutionalisation of the contemporary natural sciences necessarily raises questions that do however, go beyond the limitations of a relatively small scale empirical investigation. For example, the major finding of this thesis suggests that research is highly theoretically and technically prescribed in such a way that social considerations tend to be bracketed out of what I have termed the "context of research" of scientists. This finding is based on my research with a small sample of scientists, but the validity of the finding is strongly supported by other information that is available concerning the socialisation and general professional behaviour of scientists. Nonetheless, such a finding tests the assumption that scientists can be made responsive to policy directives, "social conscience", or any other relatively "external" demands made upon them.

For the understanding of such conditions in the life worlds of scientists must, however, go beyond the abstract invocation of concepts such as "paradigm", or "highly institutionalised", in historically oriented explorations of events long since dead and politically defused. For reasons that will emerge the sociology of science should now concern itself with the study of the structures and processes of *living* research through in depth studies of scientific life world. Given that this latter type of research is enormously time consuming and demanding of the abilities of an investigator to penetrate social and cognitive aspects of scientific "reality", a related question emerges - is it worthwhile to devote so much time and energy to substantiate theories which we half assumed in any case? The answer of any genuine investigator must be of course, "yes", on the basis of the time honoured adage that a little knowledge is indeed "a dangerous thing" - for example, as discussed above, science policy may be based on information that is quite misleading. The practices of highly institutionalised *social* scientists may however, not generally permit such deliberation over fine detail. This event merely requires further investigation in itself rather than resignation, I believe - the pertinent theoretical and methodological shortcomings of existing sociological practices and perspectives will be discussed at some length in subsequent chapters. The major theoretical limitations of the "science studies" literature have been summarised in the sub-section which follows.

The theoretical issues discussed in this thesis go well beyond the mere questioning of survey data and "common sense" beliefs about science. This first level of analysis has merely provided a stimulus for an analysis which explores in much greater depth the social reality at the basis of such generalisations. Thus after it was perceived that some in depth analysis of scientists' goals was in order a whole range of theoretical problems emerged: What is a goal? What does it mean to perceive a goal? How are goals formed? What are the social forces involved? and so on. These are some of the more specific theoretical questions that will be confronted in subsequent chapters.

1.1 Introduction to the theoretical perspective developed in the thesis

The idea that scientific research is a type of goal directed action is an idea of fundamental importance in this thesis. This point follows logically from a conceptualisation of scientific research as a particular type of social action - social action being necessarily goal oriented. The goal directed nature of social action is by no means a new idea - Max Weber and Alfred Schutz devoted considerable energy to the idea; what is new however, is the focus of this thesis on scientific research as being constituted through goal directed social action [cf. Johnston and Jagtenberg, 1978]. Thus, however broadly the net is cast across studies of science one does not find approaches which attempt in any thorough going way to treat research as a process directed towards the realisation of specific goals (at whatever level). Perhaps this is so because of a lack of understanding of the nature and significance of goals to scientific activity and action in general. The "science studies" literature may describe institutional contexts, historical circumstances, "specialty" development, individual biographies, etc., but there is to date little sense of knowledge production as being "purposive" or even possible at all within the typical scope of reconstructed events presented as analyses. To paraphrase Marx, scientists do actually work towards goals which may be sometimes only dimly perceived (if perceived at all) but which exist, nonetheless as orientations for research activities. This is what distinguishes the worst of scientists from the best of bees.

Most of the existing science studies literature can be criticised from a number of different directions. Briefly, the more important limitations that diminish the theoretical bases of this literature for analyses of the social nature of scientific action are as follows:

i. The sociology of science has tended to remain out of the ambit of the sociology of knowledge - Sociologists of science have often been reluctant to treat scientific research as incorporating the broad context of theoretical production. The understanding of the sciences have therefore often remained trapped in explanations which do not perceive scientific research as a dynamic, social process which arises in particular social, cultural, economic and political contexts. That is, scientific research and its products (such as "scientific knowledge") have tended to remain out of the ambit of the sociology of knowledge. This has been pointed out by a number of authors, for example, Mulkay [1979], Barnes [1977], and Bloor [1976].

ii. Social scientists, philosophers and historians have often reified a distinction between the "inside" and the "outside" of science - this point follows from the last criticism. As Johnston [1976] has pointed out, the popularity of a distinction between the "inside" and the "outside" of science (reified or not) has been largely responsible for the separation of scientific knowledge from the social context of its production. Specialisation in the existing division of academic labour has tended to reify the separation however, since there are still very few scholars who see themselves as "permitted" by specialty traditions to attempt to relate "inside" to "outside", or to integrate "inside" and "outside" in some form of "wholistic" analysis.

iii. Researchers have often reified an internal mechanism, or logic of procedure which drives science smoothly along cognitively rational lines - this has been a direct consequence of the reification of boundaries and particular styles of analysis. As Whitley [1972] has pointed out, the North American structural-functional sociological school of Parsons and Merton have tended to encourage an assumption that natural scientists provide a paradigm of rationality. "Non-rationality" or "bad science" arises, in this perspective, from "external" interference or "error" on the part of scientists which can be corrected by reaffirmation of established norms of science.

iv. Researchers have not adequately conceptualised science as a type of social action which involves goal direction - the major barrier here has been the general absence of phenomenologically oriented theories of science. Attention still tends to be directed away from the relationship of the individual to institutionalised structures and processes in science. In one sense it is understandable that the goals of research have been neglected by social scientists and other researchers simply *because* the natural sciences have become highly institutioanlised and therefore increasingly opaque to scrutiny from non-specialists. This relatively high level of institutionalisation (compared to the social sciences, for example) appears to obscure from meta-scientific consciousness the fact that scientific research is, like all fields of human activity, characterised by decision making processes not all of which may be fully rational or effective. Scientists, like most skilled workers, are forced to make plans from day to day on the basis of information that may be far from complete or accurate for the purposes at hand.

v. The acceptance of traditional disciplinary perspectives as being able to meet the demand for analyses of science which are of a more "critical" nature, insofar as these analyses can treat science as human product which may be affected by political and economic forces, in particular.

This thesis demonstrates that a fruitful way of confronting many of these criticisms (particularly points i, ii and iv) is through a perspective wherein scientific research is treated as an institutionalised goal directed process. In summary, until the generation of scientific knowledge is considered as a goal directed, social activity, attempts to understand science as being part of particular cultures and societies will be inadequate. That is, neither the structure nor process of science can be understood in social terms unless some account is given at a concrete and specific level of the actual goals of scientists. Such a general goal of a deeper understanding of the natural sciences may not even be without practical benefits for physical and social scientists. Thus it may help a scientist's understanding to know that a particular analysis of neural transmitters was developed with the long term goal of relieving psychotic disorders, or that a certain chemical was developed in order to defoliate bushland. And at times it may even help in the development of new knowledge to know the social, theoretical and technical goals of the original researchers whose work it may be desired to further develop. Otherwise it becomes extremely tedious trying to imagine where this knowledge might be useful, what its limitations might be, what tools might be useful in continuing a line of research, what some of the related ideas might be, what fields a new development may affect, and so on.

1.2 Goal directed action

The main focus of this thesis concerns the goal directed nature of scientific research. We have previously discussed why an analysis of the goal directed nature of science is in order, but it will be recalled that these remarks were generally made with the interests of a sociology in science in mind. There are more general reasons why an analysis of the goals of natural scientists may be of interest to an understanding of social action. These reasons will unfold from a brief general discussion of goal directed, or as it is often called, goal-"rational" behaviour.

As mentioned in the last section, Max Weber and Alfred Schutz both directed their energies to the subject of goal-rational action. Although I have found Schutz's work to be of more general use in the analysis of action developed in this thesis, Max Weber's conceptualisation of different types of action provided the initial stimulus for my theoretical interest in the subject of goal-rationality, and the constitution of science through goal directed social action.

Weber is well known for his distinction between value-rational and goal-rational activity.[4] This distinction enables Weber to characterise the bourgeois epoch as an epoch in which goal-rationality has absorbed value-rationality. In other words, modern life is, according to Weber, dominated by a mode of action which concentrates on the selection of means for the realisation of pre-given goals. The value content of these goals is necessarily bracketed in this process which is concerned with successful realisation rather than questions of value. It is hard to dispute Weber's observation of the enormously "technical" aspects of modern life, but I would submit that we do need to look more closely at the essential nature of all goals for it may be that a better understanding of what a goal is will help us to eventually transcend in theoretical and practical ways the limitations of a bourgeois world view which is so complacent about questions of value.

Of all the domains of social action, the physical sciences provide us with a particularly powerful example of what goal directed action [5] is like. Habermas, for one, has stressed that the "empirical-analytic" physical sciences are, as a cognitive mode, inherently instrumental, having a necessarily technical "cognitive interest" in the prediction and control of physical systems [Habermas, 1972], [6] or in other words, an interest in the realisation of pre-given ends. Questions of value or human understanding are ideally not part of this cognitive domain. Habermas' characterisation of the physical sciences is not particularly amenable to analysis in terms of "projects of action" as will be undertaken in this thesis, but the idea of inherent instrumentalism is suggestive in the context of the subject of the institutionalisation of research: on virtually *a priori* grounds [7] it seems likely that the mode of organisation of whole programs of research would reflect the "cognitive mode" (to use Habermas' schema) in which they were undertaken.

The overwhelmingly instrumental nature of the "sub-universe" of various research worlds is a typical assessment by scientists and social scientists, but as yet the meaning of this in practical terms has not been explained to my satisfaction. Whilst I accept the notion that the physical sciences are *generally* instrumental, that is to say, highly goal rational, as opposed to being "value rational" (in the Weberian sense mentioned above), this acceptance is not on the basis of any sound sociological evidence. This thesis has evolved very largely as an attempt to provide some empirical basis for this assumption of the generally goal rational nature of the physical sciences.

The highly ordered goal rational world of the physical scientist is potentially of very general interest since an understanding of an area of social life which is based on a highly accentuated mode of action (that is, goal directed action) may help us to perceive mechanisms and trends which are less visible in less highly institutionalised areas. An analysis of the worlds of scientists may provide a deeper understanding of social action, but even if one is more cautious about the possibilities of such an analysis, the implications of the mode of institutionalisation of the physical sciences are quite clear for the future institutionalisation of the social sciences. The physical sciences are in general a highly institutionalised and highly bureaucratised universe of meaning. In an epoch of managerial centralisation the likelihood that the social sciences will continue to evolve on a physical sciences model (at least so far as social relations are concerned) seems strong.

In saying that the natural sciences are, in a sense, a "paradigm" of goal directed behaviour, I am not merely echoing Kuhn's well known observations about the "puzzle solving" nature of "normal" science. The research goals of scientists may often be overwhelmingly technical in nature but this does not necessarily limit day-to-day research to being merely technical. Any goal may have a variety of meanings at different levels of generality - the problem is one of interpretation, and obviously depends on the evaluative context as much as the "objective" situation of research. Furthermore, any goal is only ever one goal in a hierarchy of goals, whether this be a particular individual's hierarchy that may change from week to week, or a highly institutionalised hierarchy, such as one might find as shared by members of a scientific research program. That is to say, a goal is always meaningful as one goal amongst other goals in a particular social and cultural context. These matters are, of course, not best left as merely theoretical insights since empirical research is necessary to make sense of these abstractions in terms of specific cases.

1.3 Concluding introductory remarks

In this thesis I have deliberately attempted to span (for reasons that will emerge) a number of "specialty" boundaries (the philosophy of science, the sociology of science and phenomenology), but the work of the phenomenological sociologist [8] Alfred Schutz provides a major part of the theoretical basis for this thesis. [9] In the light of such phenomenological underpinnings, "goals", for example, will be developed as objectifications of the "in-order-to" (or "pragmatic") motive which underlies all action. Goals are encountered by consciousness and "intended" as meaningful objects, which are in effect located away from the present. The further a goal is located away from the present, the more empty its "horizons of meaning" tend to be. These horizons are "filled in" by the knowledge and experiences generated as efforts are made to realise particular goals. Theoretically (and empirically) goals will be demonstrated to be context dependent, social products which, when made the subject of research, can provide valuable information about the institutionalisation of science.

Although I have not consciously attempted to restrict all the theory in this thesis to a particular school of thought, many of the basic theoretical premises derive from the phenomenological tradition (with particular attention on the work of Alfred Schutz). This is consistent with the emphasis I place on the individual subject as the source of meaningful thought and action - phenomenological sociology particularly emphasises "meaning" and "relevance" and has been most useful in my conceptualisations of the structures which constrain individual consciousness. Nonetheless, the thesis is still somewhat eclectic for I have found it necessary to supplement the more phenomenological insights with ideas drawn from conflict oriented social theory and social psychology (which are somewhat alien to the phenomenological tradition). The influences here are far too diverse to be able to label simply, but I am conscious of my debts to the ideas of Marx, Weber and Fromm. These sources have all helped to augment my understanding of human motivation, its institutionalisation, and the effects of this institutionalisation. That is to say, as a body of theory, Schutzian phenomenology has not been able to provide me with an understanding of human motivation that is sufficiently broad for the purposes of this thesis.

Although I have drawn on theoretical sources that have not so far been incorporated into the sociology of science; many of components of the theory developed in this thesis have been drawn from work that has been done in the context of the sociology of science - in particular, Richard Whitley's work on the structure of science, and Stephen Hill's work on the professional nature of science stand out.

In conclusion, as Berger and Luckmann put it in their introduction to The Social Construction of Reality: the theoretical purpose of this thesis is to engage in systematic theoretical reasoning rather than the exegesis of particular theories. Re-echoing the words of Parsons, the theoretical component of this thesis

> "is a study in social *theory* not *theories*. Its interest lies not in the separate and discrete propositions to be found in the works of [particular writers], but in a *single* body of systematic theoretical reasoning". [Quoted in Berger and Luckmann, op.cit.:29].

Furthermore, even though I have directed the analysis towards the natural sciences, the more general parts of the analysis apply to the production of all theoretical knowledge. This theoretical analysis and its empirical basis will be unfolded in the following chapters.

FOOTNOTES TO CHAPTER 1

1. In most OECD countries, more than three-quarters and much more in certain cases, of national R & D resources are concentrated in the business and government sectors; see, for example, Y. Fabian, A. Young, et.al., Patterns of Resources Devoted to Research and Experimental Development in the OECD Area 1963-1971, Paris, OECD, 1974.

2. Again in most OECD countries, R & D directed towards defence, economic, space and nuclear objectives accounts for approximately 90% of national R & D budgets; see, for example, Y. Fabian, A. Young, et.al., op.cit., 1974, note 1.

3. The OECD has provided, through its surveys and analyses, a basis for much of the science policy work that developed during the last two decades. One particularly influential document is the "Frascati Manual" which still provides general guidelines and definitions for science policy makers.

4. cf. Max Weber [1964:115-123].

5. I do not distinguish in this thesis between "goal directed action" and "goal-rational action".

6. In Knowledge and Human Interests, Habermas sets up a typology of processes of inquiry which have different "knowledge constitutive interests" - the approach of the empirical-analytic sciences incorporates a technical cognitive interest; that of the historical-hermeneutic sciences incorporates a "practical" interest in consensus; and the approach of the "critically" oriented sciences incorporates an "emancipatory" cognitive interest [Habermas, 1972:308]. Habermas' description of the physical sciences is quite accurate, but the basis for his separation of emancipation from historical-hermeneutic sciences raises theoretical issues that cannot be pursued in the scope of this thesis. For the present then, I accept Habermas' description of the physical sciences but reserve judgement on his broader efforts towards the establishment of a "critical science".

7. These "a priori" grounds are based on the assumption that the human mind is essentially projective and introjective at the same time. Thus people become like their work, as the old saying

goes. But work also becomes like the worker - to complete the circle. Or in the more mystical terms of ancient wisdom "as above so below, and as below so above".

8. I use this term very loosely since it can be argued that there is no such thing as phenomenological sociology. This argument revolves around the idea that phenomenology can be at most a philosophical proto-sociology which attempts to deal with the universal aspects of consciousness (for example, Luckmann, 1973). Without engaging in a detailed discussion about problems of demarcation in all science I would venture that such arguments ought to be regarded with some suspicion in the light of the arbitrariness which often flows from what are at best administrative decisions about academic territorial rights.

9. There are two important reasons why I have preferred to focus on Schutz at the partial expense of Weber. Firstly, as Schutz has pointed out, Weber's approach is essentially atomistic. Although Weber concerns himself with subjective meanings, intersubjectivity and hence the very possibility of social life are neglected topics [see Schutz, 1967]. Secondly, as O'Neill [1978: 203], amongst others, has pointed out, Weber makes technical rationality a fatality which obscures the fact of the historical nature and pre-conditions for the very concept (for example, the separation of knowledge, work and politics in a period of bourgeois ascendency).

CHAPTER 2: WHAT IS SCIENCE? Some Fundamental Definitions

"When I use a word", Humpty Dumpty said in a rather a scornful tone, "it means just what I choose it to mean - neither more nor less".

"The question is", said Alice, "whether you can make words mean so many different things".

"The question is", said Humpty Dumpty, "which is to be the master - that's all".

Through the Looking Glass.

2.1 Introduction and brief summary

This chapter has the purpose of providing a structure of definitions and concepts which can provide a precise and coherent basis for theoretical and practical research. Generally speaking, this framework deals with the institutionalisation of science, the nature of research and professionalism in science.

The scope of this chapter is then very broad but there are two major themes which integrate the ideas presented. Most fundamentally it will be argued that science and research are not unitary phenomena of an "external", "objective" nature that can be uniquely defined. Rather, science and research make sense at different levels - at a subjective level of meaning, at a relatively objective institutional level of meaning, and at a historical level. At the same time it will be argued that from a sociological perspective the individual scientist must be considered in the institutional context of action as a scientist. Generally speaking, the emphasis in this thesis is "contextual" - that is, the scientist is best understood as an individual acting in the context of various institutionally defined structures of relevance. Consistent with these basic themes, the logic of development in this chapter is as follows:

i. Science is both meaning and structure. Individual scientists are socialised into a highly institutionalised environment which becomes a universe of meaning. Scientific identity is formed and sustained in a process of meaning making and meaning taking.

ii. This universe of meaning is shared by other scientists. The collective activities of large numbers of scientists who share, at some level, a universe of meaning and who are engaged in the production of specialised stocks of knowledge give science the quality of a system of theoretical production.

iii. Scientific research occurs in the context of a variety of sub-universes of meaning at different levels of generality. These range from the disciplinary level to the level of the research program which is the focus of creative synthesis for the individual scientist.

iv. Scientific research is a process of theoretical production. The individual scientist conducts research within the context of professionally defined structures of relevance. These structures which provide the individual scientist with a basis for motivation, interpretation and theme, are a highly institutionalised context for scientific action.

v. Science is not an institution which permits the free expression of the individual, however. Most pertinently, scientists are subject to the social control of professionalism which ensures the continued maintenance and respectability of the scientific institution. As highly socialised beings, scientists function as productive professionals by virtue of having internalised the value of autonomy.

vi. Scientific research is not a unitary phenomenon. Differences in the orientation of research to application permit the distinction of two basic types of research - basic research and practice oriented research. These two types define the extreme ends of a continuum of different degrees of orientation to application.

vii. The institutional context of research has social and cognitive aspects. The relationship between these two aspects can be explored through an analysis of research as goal directed action.

viii. The sub-universe of the research program is not the final institutional "atom", however. Within the context of the research program action as a scientist involves both research and the legitimation of research.

2.1-1 The need for more precise definitions

Despite the fact that "science" has become a preoccupation of the age and this has resulted in the production of an enormous volume of related history, philosophy, sociology and "popular" treatments there is very little consensus about the meanings attached to the word "science" or the related concepts of various types of science (such as "pure" and "applied" science), "technology" and "research". Thus it is hardly surprising that empirical research about "science" has tended to remain at a fairly primitive theoretical level: it is often not clear whether researchers have even thought about the meaning of their basic concepts. Consequently, studies of science form a broad area of research that has often taken fundamental definitions for granted and paid a penalty of vagueness and ambiguity. Some of the more recent work in sociology and science policy (which often amounts to a kind of "common sense" sociology and philosophy of science) has attempted to be more precise about definitions, but so far the main problems persist as theoretical incoherence and ignorance about the "reality" of research. The various words used by most writers are just too abstract to make much more than philosophical sense. For example, in a recent report to the Prime Minister, "The Direct Funding of Basic Research" [Canberra: Australian Government Publishing Service, 1979], The Australian Science and Technology Council (ASTEC) has quite sensibly sifted through a variety of terms which have been used in Project Score and CSIRO reports - terms such as "research", "pure research", "basic research", "fundamental research", "pure basic research", "strategic or oriented basic research", "applied or tactical research", "strategic mission oriented research", and "tactical problem oriented research". ASTEC finally recommended that research is best characterised as "either basic (fundamental), or tactical problem-oriented, and that basic research may be subdivided into curiosity-motivated research . . . and strategic mission oriented research" [p.8]. These types of research (where research is defined as "original investigation designed to increase the general knowledge or understanding of the subject studied" [p.6]) were, in a stroke of refreshing theoretical clarity, defined as merely relative points along a continuum between "immediately applicable research" and "highly abstract research".[1] The fact that these terms are still too abstract is reflected in the examples given of "basic research projects in universities which have produced social, economic or other benefits" [Appendix 1, p.76]. To be consistent with the definitions used the examples would have needed to deal with the motivations and goals of the researchers involved in the various programs - such was not the case, however, the examples remaining as merely generalised anecdotes of a fairly ambiguous nature which, in all fairness, is probably the most that could be expected from the kind of information that is easily available to policy makers and journalists.

What follows from this example is a general point about the descriptive analysis of science is that if terms are to be used as designating different types of science or different types of research considerable care needs to be taken to ensure that the various labels employed do have some empirical validity. From the record it would appear that more attention needs to be given to the practical meaning of the various descriptive labels that have been used - for example, it is often quite difficult to know exactly how an author is distinguishing between science and technology or pure and applied science. As already mentioned, writers usually rely on the existence of "common sense" definitions which can be conveniently taken for granted. This is, of course, an unsatisfactory state of affairs, since "common sense" may conceal assumptions that are by no means shared by all sections of the community - "common sense" often amounts to little more than unquestioned prejudice. As a basis for science policy, for example, "common sense" assumptions about the discrete reality of entities such as "pure" and "applied" science may lead to very misleading pictures about the actual practices of scientists - a subject we will deal with in Chapter 3. It follows then, that it may be necessary to generate a broader empirical basis for some of these terms which do not appear to relate very precisely to the practical reality of contemporary science. As we shall see, this task is part of the analysis presented in this thesis.

In general it is true, as pointed out above by the ASTEC team, that research is not a single activity, but varies from an undirected to a highly directed process. This has always been the assumption of terms such as "pure" and "applied" research, and this assumption will be borne out in the case studies which follow. If this is so, any definition of "science" will need to be sufficiently broad to capture this diversity. Clearly, what is required at this stage if we are going to talk sensibly about science and research, are a few very general, but also precise, definitions that have both theoretical and empirical potential. By this last remark I mean that any definitions should be "fruitful" insofar as they encourage research rather than narrowing the field down to virtual tautologies.

2.2 Structure and meaning in the analysis of science

It is a great temptation for us all to assume that "reality" is an objective and invariant facticity which exists independent of the observing subject. Thus, the purveyor of social surveys may quite innocently assume that there is a "reality" "out there" that is quite amenable to measurement and quantification. Most sociologists are of course, to some extent aware of the problems and limitations of mass surveys and the other instruments and techniques which they use in the construction of sociology, but the basis for this "professional caution" is by no means sufficiently well aired. In fact, the assumption that the physical sciences are somehow "objective" and able to be adequately portrayed by the "standard" methods of observation, questionnaire administration, and literature research has tended to remain largely unquestioned, even though sociologists "ought to know better". As we will go on to discuss further in Chapter 3, this assumption has been reflected in surveys of Australian science and provides a rather misleading basis for a sociology of science, let alone science policy work towards which the surveys are primarily directed. It is not yet the time for a detailed methodological critique and thesis (as will be provided in Chapter 5) but the ontology of social reality will now be discussed in order that the basis for the sociological constructions presented in this thesis be quite clear.

Social reality, whether it be the reality of science or any other "province" of the life world, is not uniquely characterisable. As Karl Mannheim has pointed out, it is possible to distinguish between different aspects of social reality. Mannheim described three such aspects under the labels of "objective", "expressive" and "evidential" levels of *meaning*. The important feature of the objective meaning of a social phenomenon is that it can be grasped without specific knowledge of the intentions of the individuals taking part in the social process. What this implies is that objective meaning is essentially based on *shared* meanings and understandings. Expressive meaning refers to the intentions of the individuals taking part in social action.

> "This second type differs essentially from the first (objective meaning) in that it cannot be divorced from the subject and his actual streams of experience, but acquires its fully individualised content only with reference to this 'intimate' universe. And the interpretation of expressive meaning always involves the task of grasping it authentically - just as it was meant by the subject, just as it appeared to him when his consciousness was focussed upon it". [Mannheim, 1952:46].

The evidential level of meaning is an attempt to move beyond the objectified structure of a social phenomenon, and beyond the intentions of the social actors, to arrive at a type of meaning which puts the phenomenon in an historical context. Evidential meaning is constructed by going beyond the data provided in objective and expressive meaning, but it cannot be seen as just the synthesis of the latter two. Going beyond the data entails the use of generalised models to provide a theoretical expression of the evidential level, but at the same time this involves a recognition of the historical limitations of particular schematisations and the need to keep historical reality open to re-interpretation. Most sociological theory and research contains these different levels as an implicit assumption, but different schools of thought have tended to emphasise different levels of meaning in the pursuit of what they see the most fruitful kind of sociology. Thus, for example, we could contrast structuralism, which tends to proceed without much recourse to the worlds of particular individuals, with ethnomethodology which tends to proceed with more attention to the "expressive" or "subjective" level than to structures of a kind that have become more commonplace in sociology (such as "class", and "group") and also with less attention to historical analysis (than say, Marxism, which often tends to produce a kind of historically oriented structuralist analysis). Any dichotomy between "subjective" and "objective" approaches is ultimately, however, a false dichotomy since social reality has, as Mannheim suggested, *both* a subjective and objective meaning, or as Berger and Luckmann have pointed out in <u>The Social Construction of Reality</u>, society has an objective reality and a subjective reality which exist in a dialectic relationship - an individual confronts structures and processes which appear "external" and beyond his/her control and at the same time internalises and reproduces these structures in the process of finding personal meaning and identity in the world. In the terms of the general theoretical approach adopted in this thesis an individual acts within the confines of a "finite province of meaning" which is institutionalised as various structures and processes. These "structures" and "processes" are not however empirical objects as material entities are for the physical sciences; nonetheless they are real objects in an observer's perception. Without the presence of structure we could not perceive a meaningful world but only a disconnected series of unrelated social events ("social things"). This meaningful world which forms the essential matrix for being and action is not static, however. The world is in flux and consequently we perceive change. Insofar as the changes in a structured world are not random we can speak of "process" as indicating meaningful transformations in a structured world.

These considerations are of particular relevance to the way in which an analysis of science is to be pursued. Historically speaking there has been a great tendency in the sociology of science to emphasise the more "objective" structural aspects of science. Some of the manifestations of this one-sidedness have been briefly summarised in Chapter 1 - sociology of science has tended to remain out of the ambit of the sociology of knowledge; a distinction between the "inside" and the "outside" of science has been reified; researchers have assumed scientists to be cognitively rational. Thus, for example, in the North American Mertonian tradition there has been considerable attention paid to the organisation of scientists in different kinds of contexts and the kinds of norms that the occupation of science apparently requires. Scant attention has been paid, however, to the actual constitution of individual scientific consciousness and the way in which subjective and objective levels of meaning are related both in the "world" of science and the conduct of sociological research about that "world". Consequently, the major concern of this thesis has been the generation of theory and methodology which is fully aware of both subjective and objective aspects of social reality. There may be a variety of political-economic reasons for the above-mentioned historical emphasis on the "objective" nature of science (not the least being the need to "manage" large capital intensive systems of production) but the essence of the problem as treated in this thesis is the fundamentally instrumental nature of bourgeois capitalism (not to exclude other forms of contemporary social organisation, however). This subject has already been introduced in Chapter 1 and will be further explored in Chapter 4.

From a theoretical point of view, I have in this thesis attempted to synthesise a number of hitherto rather unconnected concepts and perspectives. As mentioned in Chapter 1 though, I am particularly indebted to work of Alfred Schutz and Berger and Luckmann. The more phenomenological perspective of these authors has particularly inspired my concern with the structures of individual and collective scientific consciousness. [2]

In the following sections of this chapter this synthesis of concepts will be developed in the form of a series of related definitions of science and research. Science is defined as a professionalised system of theoretical production *and* a "finite province of meaning" [Berger and Luckmann]. That is to say, science is at the same time a social system with definable structures and processes and a world of meaning for individual scientists. Research is defined as a process of creating and transforming objects of consciousness by certain procedures in the context of "thematic", "interpretational" and "motivational" "structures of relevancy" [Schutz]. Research occurs as part of a larger system (science) which is more than just an aggregate of a number of different "programs" of research. Research is just one aspect of the process of production and reproduction of a structured world of science.

2.3 Science and its sub-universes of meaning

Consistent with the "interpretive" phenomenological perspective that is emphasised in this thesis, we start our analysis of the world of science with an investigation of the nature of science as a universe of *meaning* for individual scientists.

Whilst a scientist is engaged in the various phases associated with the production of scientific knowledge (e.g., speculation, experimentation, documentation, etc.) he can be said to be participating in "the world of science" which, as Schutz and Luckmann [1974] and Berger and Luckmann [1966] define it, is a "finite province of meaning",[3] characterised by a harmony and compatibility of experience lived in a particular "cognitive style" [Schutz and Luckmann, 1974:23, 24]. This is a very broad location; most scientists actually do research in particular disciplines, specialties and research programs, which could all be defined as "sub-universes of meaning" [Berger and Luckmann, 1967:102] of a decreasing scope. According to Schutz and Luckmann [1974:35, 36] cognitive styles can be distinguished along several dimensions: tension of consciousness (e.g., "wide awakeness"), characteristic epoche (e.g., natural attitude), form of spontaneity (e.g., gearing into the external world), form of sociality (e.g., intersubjectivity), form of self experience (e.g., "bound" into the role of chemist and "free" to experiment with particular chemicals) and temporal perspective (e.g., "standard" time). What particularly distinguishes a scientific cognitive style from an every-day life-world cognitive style is everywhere assumed by Schutz and Luckmann and Berger and Luckmann to be the characteristic epoche of the "scientific attitude" (and whilst this seems a reasonable assumption, it *is* just an assumption which could do with further exploration). "Scientific attitude" is not over clearly defined, but implicitly it is a type of theoretical attitude, where scientific things are no longer taken for granted as scientific problems are encountered and dealt with in a scientific way. The way that things may be dealt with scientifically is obviously subject to huge variation dependent on discipline, individual style, etc.; at a high level of generality one might speak of scientific "rationality", but how successfully one can distinguish scientific rationality from other rationalities in an empirically useful sense is not fully clear. For example, Garfinkel [1975] has provided an inventory of "rationalities" which distinguish the "attitude of scientific theorising" from the "attitude of daily life" - attention to (i) the compatibility of ends-means relationships with principles of formal logic, (ii) semantic clarity and distinctness, (iii) clarity and distinctness for its own sake, and (iv) compatibility of the definition of a situation with scientific knowledge, do not occur in the attitude of daily life. But, Garfinkel is careful to point out that "to avoid misunderstanding I want to stress that the concern here is with the attitude of scientific theorising. The attitude that informs the activities of actual scientific enquiry is another matter entirely" [p.62]. Garfinkel leaves no clue as to what this other "real" attitude might be - a somewhat ironical omission for one so apparently concerned with "life world" which has been treated in so cavalier a fashion by excessively theorising sociologists. Although the matter will not be further explored along the lines suggested by Garfinkel, it is important for the sake of clarity (a properly scientific concern!) that my assumptions on this matter be expressed. I do assume that there is in fact no mysterious "third attitude" that governs the behaviour of scientists, but simply that scientists alternate, at an unknown frequency, between the attitude of daily life, and the attitude of scientific theorising (that is, the theoretical attitude as defined above).

Whilst the concepts of finite universes (or "worlds") and sub-universes of meaning are extremely useful as a way of understanding the world of science there do appear to be some shortcomings in the Schutzian formulation of the notion - for example, the problem of change is fairly lightly dealt with and the whole issue of whether provinces of meaning are necessary structures (i.e., with an ontological basis) or heuristic devices (such as "ideal types") is apparently bracketed. The concept of sub-universe of meaning is not discussed at great length in either Berger and Luckmann [1967] or Schutz and Luckmann [1974] and there is certainly some need for theoretical clarification - for example, Berger and Luckmann stress "role specialisation" as being at the basis of "institutional segmentation", but as Schutz and Luckmann discuss the general notion of "cognitive style" much finer distinctions appear to be possible, viz., the "aspects" of "tension of consciousness", "characteristic epoche", "form of spontaneity", "form of socialty", "form of self experience", and "temporal perspective". The extent to which these parameters can vary before any particular "accent of reality" is withdrawn (causing a subsequent "alternation") has not to my knowledge been thoroughly investigated. That is to say, issues of demarcation, such as the precise basis for distinguishing between "sub-universes" within a particular finite province of meaning, remains to be further clarified.

The idea that science is somehow structured into various sub-units (or sub-universes) is of course commonplace. The meaning of terms such as discipline, specialty and research program do not however tend to be well defined by those who use them. Some attention will be given in this section therefore, to the definition of these three terms, particularly since they will be the main units of organisation referred to in the empirical section of the thesis. These terms are however, only part of a range of concepts that have been introduced to the sociology of science in some attempt to deal with an apparently diverse mode of organisation of contemporary science.

The "paradigm" [Kuhn, 1962] and the "invisible college" [Crane, 1972] have probably received the most attention in the literature, but given the widespread recognition that these concepts do not adequately account for the social reality of all science other concepts have been developed more recently. [4] Whitley [1976] for example, devised the notion of a "research area" as "collectivities based on some degree of commitment to a set of research practices and techniques". The research area is a smaller unit of organisation than the discipline and specialty which he defines as follows:

> "While not as directly connected to current research work as are specialties and research areas, disciplines are the units which relate such work to other activities and structures. Through their development as educational units of organisation, disciplines constitute the overall social and economic framework for scientific activity". [Whitley, 1976:494].

> "'Specialties' are focussed on explanatory models and definitions of the phenomena under consideration. Membership of specialties implies commitment to particular types of accounts and preferred ways of formulating the underlying object of concern". [Whitley, 1976:473].

While Whitley's continued attempts to define, over the last five or six years, an overall framework for the analysis of the structure of scientific activity represent a considerable advance in the sociology of science (which cannot be praised for its theoretical coherence) there are still problems involved with his conceptualisations. As he recognises [Whitley, 1976:473]

> "(Many scientists) engage in largely *ad hoc* patterns of associations which are structured more by organisational and everyday technological exigencies than by strong normative beliefs . . . The institutionalisation of specialties and research areas is not always very high and, in terms of frequent interaction and intense common commitments, may be non-existent. This lack of solidaristic communities with developed cognitive commitments seems to be an increasingly important aspect of the contemporary sciences".
> [Bitz, et.al., 1975].

In other words, the units of organisation imposed by sociologists (in particular) may actually suggest more order than does actually exist in science.

A more serious criticism of Whitley's system though, is that it does not provide a fully adequate description of scientific research. The idea of research area is far too vague to be of great use in describing the typical concrete research situation. If we are interested in describing the day-to-day process of research we need to account more specifically for the way that scientists tend to work together as a team towards shared goals. For this reason the concept of "research program" will be used in this thesis as that unit of organisation most directly linked to the process of research. By research program I mean that sub-universe of meaning constituted through the collective activities of a group of researchers and support workers who share a commitment to particular research practices and techniques (this follows from a consideration of scientists as belonging to disciplines, specialties and research areas, as defined above), and who are directed towards a shared set of goals. That is, a research program is composed of a group of researchers and their assistants, who as a team are working towards shared goals and who share, to some extent, a common stock of specialised knowledge. It is taken for granted that within these shared horizons a division of labour will be a necessary feature of such collective activity. In other words, a research program is characterised by the co-presence of mechanical and organic solidarity (in the Durkheimian sense). In addition, as is sometimes the case, a research team may be geographically dispersed to some extent, but this need not prevent the mutual orientation of research.

In summary then, although Whitley's definitions of disciplines and specialties are adequate as descriptions of these more abstract realms of scientific reality in which scientists participate, they do need to be balanced by a consideration of those processes which provide the actual basis for and sustain the "higher level" structures of the scientific life world. The concept of research program has been introduced precisely with that end in mind. This concept is still vague in a number of different respects however, and requires further clarification.

In the first instance, although the main object of any research program is the production of professionally accredited knowledge, the research program is in fact only the point of origin of knowledge claims. Beyond this point of origin of research findings and their presentation to scientific peers many social processes of selection, and perhaps modification, occur - that is, in the process of publication and reinvestment in further research, social processes mediate with the interests of creating a consensus about the validity and fruitfulness of the products of the research program.

Furthermore, given the existence of research programs (as defined) it is apparent that there is a certain ambiguity involved with respect to Whitley's definition of research area: clearly a research program may be a research area, but a research area may be more than a research program. Furthermore, if a research area is just a collectivity of similarly directed research programs where does one draw the line between a research area and a specialty? As the concepts have been defined above, one obvious point of demarcation between research program and research area is the level at which collective action occurs. The research program is a unit of organisation centred around a primary group most likely engaged in face to face interaction involving the sharing of research goals of a variety of levels of generality. And, insofar as the research program is the locus of practical activity, this sharing necessarily extends through the metaphysical and theoretical levels right down to the technical level of activity. That is, there is rather more than "some" degree of commitment to particular research practices and techniques implied in the concept of research program. In a research program individuals' commitments may vary as a product of a division of labour, but there remains nonetheless a commitment to other members' practices and techniques since day-to-day research is constrained by the activities of the other members. A research area, on the other hand, can continue to exist even if individual scientists only communicate through the literature. That is, collective action is not defined in the research area on the basis of day-to-day research.

A further point of clarification about the definition of a research program concerns the nature of a research program as a primary group of individuals. The question arises as to whether the activities of an individual scientist working alone could qualify as a research program. As we will have occasion to discuss again later, this situation of the solitary scientist is becoming untypical in modern science which because of its capital and labour intensification increasingly requires co-operative work, particularly as research becomes more oriented towards the market place. Nonetheless, solitary scientists continue to exist (probably with greatest numbers in more theoretically oriented, university-based research), and it would be somewhat arbitrary to deny the possibility of the existence of a similarly structured research world for these individuals. Clearly, the main difference concerns the nature of research as constituted through individual or joint action (or projects of action, as defined in Section 4.4). By definition, a sub-universe of meaning is socially sustained, and so the question becomes one of whether the sub-universe of research of the solitary scientist could be the same as that of a group of researchers. The answer would appear to be obviously "no", since the sub-universe of the solitary scientist would be more highly constituted through relationships of an *anonymous* nature - i.e., through the literature, through correspondence with individuals that are not seen on a daily basis, etc. Or in other words, the sub-universe of research of the solitary scientist becomes more closely approximate to that of the "research area". Nonetheless, given the obvious structural similarities between the sub-universe of the solitary scientist and the team scientist - for example, in both cases research occurs in a structured cognitive field oriented by professional orientational reference groups (see Section 2.8), it seems sensible merely to restrict the term and speak of an "individual" research program.

The above descriptions of a research program as a "sub-universe of meaning" needs to be qualified in order to avoid the inevitable criticism that such a unit of organisation sounds suspiciously like a scaled down version of a Kuhnian "paradigm".[5] In response to this hypothetical criticism I take it for granted that the notion of sharing (horizons of meaning, etc.) that characterises a research program is not necessarily exclusive, exhaustive or unambiguous. Thus two scientists at work on the same program may not share all meanings or all goals; in fact, it seems virtually impossible that this could be so. Apart from the fact that individuals have necessarily had different biographies (which seems to preclude the possibility of completely identical experiences and understandings) contemporary scientific research seems to be fairly universally set within elitist, authoritarian and sexist structures [see, for example, Rose and Rose, 1976a and 1976b, and Easlea, 1973]. The presence of such structural inequalities (and some of these appear unavoidable, as in the case of a scientific "apprenticeship" which can hardly be conceived as a relationship of equals in all senses of the word) are preconditions which severely restrict the amount of sharing that is possible in the "sub-universe" of the research program. In other words, "sharing" is a necessarily vague term which merely denotes a certain overlapping of individual meanings and goals that is constituted by virtue of (and despite) social constraints of a fairly specific nature (for example, being a *professional scientist* at *work* in an *organisation*). As the point of origin of knowledge claims the research program represents a point of potentially creative synthesis. The research program represents a point of flux where stocks of knowledge that may derive from a variety of specialties (via the expertise of individual members of research programs) are reassimilated in the process of research.

Consistent with Schutz's general approach to social reality, the idea of science as a universe of meaning starts from the world taken for granted as providing a general field of open possibilities, that is, a field where options have not yet been "weighted" and made to contest with each other, and where choices have not yet been made. Although this field is constituted as a historical product of prior actions and constraints of various types, it is experienced as a "natural" flow of events which constitutes our being from moment to moment. The individual does however, experience a "situational logic" which is a product of a particular biography and various institutionally defined structures of relevance (motivational, thematic and interpretational relevancies - see Section 2.6).

In passing, we should note that these last remarks are as true for a scientific researcher at work in a laboratory, or at a desk, as it is for the individual going about the world of "everyday", "common sense" reality. In both situations there are worlds taken for granted, fields of open possibilities, problematic situations, and so forth. The particular mediating influences on action, the particular goals and means selected or imposed are always tied to concrete situations but here it is important to see the particulars as related by virtue of being set within structurally similar horizons of meaning. This subject will be taken up again in Chapter 3 where the general nature of the goals of scientific action will be explored.

In this section the basic units of organisation of the world of science have been defined as sub-universes of meaning. This section has been particularly important in providing a phenomenologically inspired theoretical structure for future reference. Disciplines, specialties and research areas were defined as universes of meaning which provide a context for that point of creative synthesis and most "concrete" of sub-universes, the research program. In the remaining sections we will focus attention on the activity of scientific research and the way in which individual scientists become subject to professional control. In the next section we will look more closely at the main product of research, that is theoretical "knowledge", by way of a discussion of the nature of science as a system of theoretical production. As we have already discussed, structure and meaning are inseparable in any analysis of process in the life world. The section which follows is then the necessary complement of any discussion of science as a universe of meaning.

2.4 Science as a system of theoretical production

The concept of science as a social system has been well discussed in the literature and hardly needs a detailed explanation and justification at this point (see, for example, the work of Parsons, Merton, Storer, and their numerous critics). What follows will be then a rather brief account of a subject that has been more than adequately documented.

One of the essential features of a system is that it is constituted through three inter-related processes: those of differentiation, integration and reproduction. Furthermore, this is also true of sub-systems (however weakly defined) within any system (such as disciplines, specialties, and research programs within science). Again, the ideas of differentiation, integration and reproduction are basic to sociology and do not need a detailed explanation in the present context. Briefly though, differentiation ensures the autonomy of a social system and demarcates it in relation to other areas; integration creates stable inter-relationships for projects of action or labour processes in general; reproduction involves the recreation of structures, particularly through recruitment of new members such that the system will persist through time.

The other basic feature of a system that is being emphasised here is the productive aspect of organised social activity. Clearly, consumption and production are related, but there is some reason to differentiate systems whose *consciously held* overall objective is production from those whose objectives are more consciously to do with consumption (for example, the advertising industry, the media and the family - but clearly this is not a clear-cut distinction for in all these examples consumption depends on the production of various commodities, not least of which are socially conditioned human beings). In the case of science, theoretical production is the primary aim that is consciously held by scientists, the consumption of other knowledge and materials being the means to this end. The way in which the products of science are consumed is still a matter of vital importance however, and this subject will be taken up again in a subsequent section on the *professionalised* nature of this system of theoretical production.

In this definition of science as a system of production we have obviously not restricted our considerations to science as "knowledge" only, the reasons being that such a typically philosophical preoccupation is unnecessarily narrow in scope and would entail a severe mismatch with much of the sociological and popular literature which sometimes means "just" knowledge, but often goes on to mean other things at other times. "Knowledge" is a product of science, but only one such product – ignorance, as well as techniques and instruments also being other products.[6] The point being emphasised here is that science is a system of theoretical production some of which may be "true" and some of which may be "false" (by whatever criteria one cares to nominate). The essential quality of theory is that it is only a more or less helpful way of dealing with an otherwise chaotic world. Theory provides us with empirically testable conceptual "maps" of the world.

In this definition of science the role of "scientific method" has likewise been reduced in status. The methods that scientists use in the pursuit of their craft are of course vitally important for a thorough going understanding of the world of science. But that is not to say that the essential quality of science could be known merely through a study of the various methods used in the production of scientific knowledge. On what basis do we separate means and ends, process and structure, act and context, after all? Obviously, we effect such separations on the basis of prior notions of relatively "natural" divisions that occur in perception and in common sense "logic". As a sociologist one may at times make such assumptions, but at the level of analysis pursued in this thesis it would be theoretically counter-productive to knowingly begin with such assumptions, particularly when one's own experience indicates problems with conventional definitions. So, at this stage I am certainly not yielding to the line of thought which would seek to define science by the methods it employs (or should employ - such as is the approach of Karl Popper, for example). "Scientific method" is, furthermore, a rather unknown quantity. We have many prescriptions but very few descriptions of the methods *actually* employed by scientists. And indeed, some of the evidence seems to indicate that scientists may in fact be highly unscientific by the traditional criteria of what it is to be a good scientist [see for example, Lakatos, 1970, and Feyerabend, 1975]. "Scientific method" will be approached as being simply part of the process of production of scientific knowledge and an aspect of the "institutionalisation" of thought.

The system of science has both cognitive and social aspects - that is, science involves both the organisation of thought processes *and* social action (the two being related) as part of the general institutionalisation [7] of the system. That is to say, the patterns of meanings and action which constitute a system at any given moment may, for the purposes of analysis be separated into cognitive and social aspects (which I will go on to deal with in terms of cognitive and social institutionalisation). Now, of course it is important to be able to make analytical distinctions such as these but we should not forget that it is just as much the task of analysis to put the pieces back together as it is to take them apart. Since the thoughts and actions of individuals are necessarily constituted in some institutionalised context the task of linking the cognitive and social aspects of the analysis in this thesis will be dealt with by focussing on the way in which social forces are involved in the constitution of individual consciousness. Individual consciousness has been approached from two directions. On the one hand I have identified shared structures of goals, theory, techniques and beliefs which provided contexts for and constraints on individual consciousness. On the other hand, I have identified particular forces that were important in the process of formation and orientation of these structures. These forces which have been particularly relevant to the understanding of scientists' thoughts and actions consist of "professional" forces and generalised "external" demands. In this context individual action has been conceived as basically goal directed. One of the main theoretical problems that follows from this perspective is the development of an understanding of the way that individuals internalise and orient themselves towards goals which are at least partially constituted in the context of forces which extend beyond individual consciousness. This problem will provide a continuing theme for this thesis.

2.5 Science and professionalism

Professionalism is a concept of major importance in accounting for the behaviour of scientists and the general character of the structures which dominate the world of science. The universe of meaning of an individual scientist is largely sustained and constituted through professionally oriented interaction - this interaction certainly functions as a support for individual egos, but it is nonetheless in the first instance a form of social control which provides the basis for continued interaction and the smooth functioning of research processes. In other words, professionalism is a social force which is sustaining and constitutive of both individual universes of meaning and social systems.

More generally speaking, an accent on social control is particularly effective as a way of socially relativising science, which becomes similar to the more traditionally identified "professions" such as the law, medicine and engineering. This concept of "professionalism" as a form of social control derives from the work of Terence Johnston [1972] and is not to be confused with the concept of "profession", by which I mean a collectivity of people in similar or related occupations which develops or uses a particular defined branch of knowledge over which the collectivity exercises some degree of hegemony [cf. Hill and Jagtenberg, 1977:35].

The concept of professionalism emphasises a structural aspect insofar as social control depends on structures of inequality with particular social groups occupying positions of dominance within a division of labour, but as will become apparent this structural aspect is not separable from cognitive considerations. That is to say, professionalism implies the control of knowledge (or what is accepted as "knowledge") and the generation and institutionalisation of relatively autonomous sub-universes of meaning which are necessarily restrictive and highly impenetrable to non professionals by virtue of their being constituted through the active sharing of specialised stocks of knowledge [cf. Freidson, 1970]. Professionalism is then a force which affects the nature of science as province of meaning and system of production - professionalism means the control of meanings and the structures and processes through which the world of science is constituted. This control process necessarily involves the legitimation of particular sub-universes of meaning as more or less "marginal" to established scientific interests, or what one could call the scientific "main-stream" - this concept of marginality will be developed further in Section 2.6. We can summarise all this by speaking of science as "professionalised".

As Johnson points out, an emphasis on social control permits some transcendence of the limitations of two tendencies which have dominated the literature on professions: an a-theoretical "trait" approach which attempts to list the central core elements in a rather diverse literature, and an a-historical functionalist approach which can see no alternative to the existing social system in which professions function as an important source of social order. The adoption of this definition is intended furthermore, to separate the analysis of science as profession in this thesis from the narrower concerns of more functionalist sociologists of science with questions such as the consequences of the growth of bureaucratic scientific organisations for the "creative" role of scientists - e.g., Marcson [1960], Kornhauser [1962], Glaser [1963], Hagstrom [1965], Hirsch [1968], and Cotgrove and Box [1970].

As indicated above, the similarity between science as a profession and other professions rests ultimately with the fact that it too is constituted through a type of occupational control. This is still, obviously, a very broad definition which needs further specification. Following Johnson's [1973] analysis, professionalism is a type of "collegiate control",

> "in which the producer defines the needs of the consumer and the manner in which these needs are catered for . . .

> Professionalism arises where the tensions inherent in the producer-consumer relationship are controlled by means of an institutional framework based upon occupational authority. This form of control occurs only where certain conditions exist, giving rise to common characteristics in organisation and practice". [op.cit:45, 51].

Thus, science is essentially like the more traditionally accepted professions such as medicine, law and engineering in its basic social character. [8]

The key *difference* between science and other professions lies in the former's inward focussed attention to certification of the knowledge it claims, and the latter's client-focussed attention which attempts to attain uniformity in presentation of the profession's knowledge to the outside world. But what happens in the socialisation of an aspirant into the profession and in professional dialogue is essentially similar. It is true that Johnson does not specifically deal with the occupational mode of science, per se, [9] but it is being argued here that his analysis is still compatible with the idea that science is constituted through professionalism. Let us consider, then, the main objections that could be raised against the idea that scientists exercise "collegiate control" as defined above.

The most serious objections that could be raised against this argument are firstly that science does not have clients, and secondly that even given a customer-client relationship, scientists do not define the needs of the consumer and the way in which these needs are catered for. In reply to the claim that science is different from other professions because science has no clients and other professions do, I would suggest that indeed science does have clients. For how would fundamental science survive unless the enterprise were supported in a variety of government and business-sponsored institutions, each of which are "clients" for some benefit from the research. However, as Johnson observes, what happens inside the profession and the way "professional" knowledge is used depends on the type of client the profession in general relates to. For example, professionals in engineering and accountancy are most often employed as problem solvers in private enterprise or government organisations which retain substantial power over the activities of its professional employees; the medical and legal professions, on the other hand, usually have individual members of the wider community as their clients and so are less subject to direct influence over their behaviour as professionals. The clients of science are first the government, thence industry and, indirectly, the general public: [10] while the general public, rather than the particular government, is seen by those in government as the clientele receiving the long term benefits or liabilities of science, the science professions, by virtue of being some levels removed from the general public, are usually quite insulated from the mainstream of "everyday" social life. [11] But government and industrial objectives do impinge on the character of science, and certainly on the areas into which research is conducted.

In Australia, for example, expenditures by *government* on research that could contribute to economic growth outweighed expenditures on ecological research (some of which evaluates the consequence of this growth) by 28 to 1. A similar sort of impact is registered on any other profession simply depending on who the clients tend to be.

In addition to these "external" clients scientists also have other scientists as clients, even though the nature of this "internal" exchange relationship is obscured by the mediation of a relatively impersonal publication process. Although the overall goal of all scientists (and their communication processes) may be the generation of new knowledge with the implication of a large joint project as opposed to an internal customer-client relationship the fact of a highly specialised division of labour within science clearly implies relationships of functional interdependency between scientists. That is, scientists necessarily relate to some of their peers as sources of needed information which they often do not and cannot question because of the separation of expertise.

The second objection does not stand up to scrutiny either for scientists do define consumer needs and the manner in which these needs are catered for. The key here rests in the instrumental nature of scientific knowledge as it is used in the solution of consensually defined problems. In the case of contract research, which provides the most obvious example, it is generally the scientist's prerogative to redefine a given problem in terms of the specialised tools at his/her disposal. For example, an air pollution problem becomes a problem of measuring and controlling the concentration of particular known chemicals in a particular atmosphere. In all other cases where customer and client may be in a more negotiable relationship the quantity and quality of knowledge and other shared resources will eventually prevail such that negotiation will stop when finite and specialised individual scientific domains cease to overlap with respect to particular "shared" problems. At that point needs and means are defined by the prevailing specialist/s who necessarily remain as ultimately authoritative. [12]

If there is any remaining doubt as to the professionalised nature of science we need only to briefly review the socialisation process which aspiring professionals undergo. Johnson outlines some of the typical processes as follows:

> "Occupational norms are inculcated during lengthy periods of training. The assimilating institutions are characterised by close supervision within an apprenticeship system and peer-solitarity through the creation of vocational schools which are directly or effectively controlled by practitioners. Associational forms of organisation, a developed network of communication and a high level of interaction through branches, discussion groups, journals, 'social occasions', etc., all help to maintain the sub-culture and mores of

the occupation . . ." [op.cit:55].

The fact that Johnson drew this material from a study of Canadian medical doctors by Oswald Hall only serves to emphasise the point being made here: the socialisation of aspiring scientists is not qualitatively different to that of any other aspiring professionals, doctors, engineers, who-ever.

So far in our treatment of the way that professionalism acts as a constraint on scientific thought and action we do not yet have an adequate appreciation of the way that professional values are internalised nor indeed what the central values in this professionalism might be. In the remaining parts of this section I shall, therefore, introduce some notions of reference group theory, and show how autonomy functions as a central value for professional scientists. Some understanding of the origin of scientists' concern with autonomy is particularly important for an understanding of the images of science that prevail in much of the literature about science.

The collectivity of people that constitute any profession do, over time, provide the successfully socialised professional with an "orientational reference group", that is, a reference group which is meaningful but separate from immediate experience, and abstract rather than composed of particular known individuals. Such a group tends to provide the behaviour *orienting* categories of norms, values, beliefs, etc., for the individual, but it is being claimed here that what is particularly significant about the orientational reference group is that it provides a deeper level of personal orientation than the particular organisations through which an individual may pass as his/her job changes. The "professional" orientational reference group is effective at a deep level in personality and cognition and plays a major part in the production and reproduction of the cognitive structures that are internalised by scientists (see Section 2.7). These structures which provide the basis for individual and collective scientific action are formed and reformed as a product of the continued experience of working as a professional. The concept of "professional orientational reference group" will be further developed in Chapter 4.

2.5-1 <u>The role of autonomy in the profession</u>

All professions place a central value on autonomy for their members, and there are several social reasons for this.

First, a profession cannot exist unless the knowledge it controls is wanted, in whatever form it is dispensed, by the wider community. Depending on how valuable "their" knowledge is seen to be by the wider community, professions (and their members) tend to achieve prestige in the eyes of the general public. As long as those outside the profession believe that because the individual *is* a professional he will act responsibly in "taking care" of professional knowledge and in dispensing his expertise to them as clients, then the members of the profession will be allowed to act autonomously. In other words, the wider community must continue to believe that while professionals do act autonomously, they will always act as professionals. Furthermore, in our society people generally value freedom to do what they want, and thus tend to award prestige to others who appear to be more in control of their own decisions than themselves. The prestige of professions, and ultimately the autonomy (and remuneration) its members can enjoy, suffers if the wider community starts to believe that the profession's knowledge is not so valuable any more, or that the profession offers no guarantee that its members will, by and large, act as responsibly as each other. An example of a profession in decline over the last century is that of the clergy, which lost its power over governments as more and more people (and institutions such as governments) became aware they could do without its particular knowledge and services; examples of occupations which are presently reaching towards professionalism but still must gain further credibility, include personnel management [cf. Horne, 1976] and osteopathy.

Thus by its very character a profession is particularly attractive to potential and current members because of its promise of autonomy. [13] In Australian science Hill et.al. [1974] found autonomy to be the most central value to several groups of scientists studied in both academic and industrial research institutions. Furthermore, research students tended to believe that the most attractive employment was to be found in organisations that offered the most autonomy. [14] But to protect this autonomy professionals must look to their borders for, ultimately, their survival (and thus the survival of individual autonomy and prestige of their members) depends on maintaining an appearance of unity within and avoiding control from outside. Science is no exception - there are numerous examples which can be drawn from the history of science to illustrate the point. The highly defensive responses of British scientists to the proposals in the Rothschild Report [1971] and Australian scientists to the proposals in the Philip Report [1975] provide recent examples (which will be reconsidered in Chapter 3). Other examples are Polanyi's response to threat of outside encroachment on science during the 1930's, [15] and response of the scientific community in the 1950's to Velikovsky. [16] This is also evident in the tenor of self-justification of such reports to government as the Australian Philip Report, which Ronayne [1976] contends is "the most recent example we have of the rearguard action being fought by scientists against the encroachment by government into the area of decision-making in science, an area which is traditionally the preserve of the scientists themselves".

To recap the main function that a belief in autonomy has in holding a profession together is that members believe that when acting as professionals they are acting to please themselves. Therefore, a member will continue to act "professionally" even when he is not in the immediate company of another professional.

With the sheer size of contemporary society and the large number of separate sources of professional employment within it, scientists (in particular) may be relatively isolated from each other - scientists may be spread throughout a variety of institutions in almost every country of the world. Except when the isolated scientist establishes contact with other professionals, e.g., through conferences and publication, the *profession* has no means of ensuring that members will follow professional maxims of conduct or professionally sanctioned approaches to research. Thus, when a professional is employed by an organisation for some mission, the only test of professionalism available to the organisation *may be* just a pragmatic one - can the person do the job? That is, what is immediately accessible is not the code of values which may be influencing the professional, but the person's instrumental value to the mission at hand. It is thus certainly *possible* that what passes for "professional" behaviour may in fact be very sloppy workmanship, the inefficiency of which is masked because nobody else in the organisation is really competent to evaluate it. This can happen even when other scientists are employed there as well: for example, research supervisors may be afraid to critically evaluate the research of their subordinates, particularly when their subordinates appear to have a greater mastery of their own specialty than do their supervisors.[17] That is, professional rewards and sanctions are not enough to keep all professionals in line because (a) they can only touch those who are presently interacting with the profession; (b) the profession usually can only sanction quite extreme deviations from 'professional' behaviour. A profession will usually, in the interests of unity, close ranks around those accused by outsiders, anyway. It is ironic that generally these are the only incidences of misconduct which come to the attention of professional associations; (c) profession rewards in science can only be offered to those who *present* the finished products of their labour - which are embodied in research results and publication - to a professional audience. What happens *during* the research remains outside the scrutiny of professional evaluation.

The inability of professional control to *directly* influence the research behaviour of scientists is further exacerbated by a widely evident attitude amongst a significant number of scientists that personal rewards lie primarily in the research itself, that is, in successfully solving the problems that they, or others, have set. Professional colleagues, as well as the "local" institution for which they work, are of secondary relevance as audiences for whom their professionalism is being acted out. This orientation, called "instrumental" by Box and Cotgrove [1966] was found to be quite central to scientists working in a number of industrial research organisations in Australia - see Hill [1974].

An important upshot of the role of autonomy in the profession is that unless scientists want to act as professionals - that is, unless scientists want to be autonomous, or partly so, in order that the "unproblematic" and largely "invisible" operation of the norms of professional conduct can be presumed by the scientist and his audience to apply to all other scientists - then the profession itself is likely to crumble in the face of divisive criticism and control exerted from outside the profession. In this sense the association of autonomy with being a professional is quite central to continuation of the profession itself.

There is one essential question still to ask about the social functions of autonomy. This concerns how it is that professionals in general, and scientists in particular, come to associate who they are and how they *want* to act with being a professional. Here again "autonomy" plays a social role - as a value which research students wish to achieve for themselves and which, therefore, influences what they look for in their research training. On the basis of studies of Ph.D training in Australia, Hill et.al. [1974] found that what students appeared to be looking for in their training was very much influenced by the promise of what science, as a profession, offers in providing a secure identity for oneself. By identifying the profession of science with who they want to be, students come to accept what they understand to be values of the profession as their own. Students develop a concept of the science profession, however, as that of an inward-looking institution, more concerned with certification of the knowledge it claims than with its use for the benefit of its clients. So the professional image with which students tend to associate their own identity as scientists is one that is rather insulated from the world outside science. In science particularly, being a professional usually means becoming a specialist. The identification by students of themselves as professionals with the narrow band of a discipline they have mastered, doubly reinforces the insularity of the graduating professional - (a) from wider sources of knowledge and understanding, and (b) from activities which may enjoin his professional research behaviour to its application in the service of an applied mission.

In short, autonomy is important as a social value since it provides a means by which scientists identify their own desired action with professionalism in science; it provides a means by which they learn to *share* expectations about scientific enquiry - its aims, its methods, and the criteria by which knowledge is accepted and certified as valid. However, in forming part of the social fabric which enables scientists to function, the internalisation of the value of autonomy also serves to preserve the inward focussed and predominantly cognitive orientation of science as it is traditionally portrayed during professional socialisation. That is, the value of autonomy functions in science as an agent of occupational control and as such is inherently conservative of the scientific status quo. This tendency towards a cognitive orientation that is a condition of professional autonomy is not without its problems for scientists, however. As we will see in a later case study (Chapter 6) when scientists have to contend with the marginality of their research they may fall into a "double bind" situation of having to be both perceived as professionally respectable and socially useful, a situation of not necessarily compatible alternatives.

2.6 Research

Research is usually defined functionally, that is in terms of what research is supposed to produce. A typical definition is, as we have already seen, that research is "original investigation designed to increase the general knowledge or understanding of the subject studied" [ASTEC, op.cit]. Or in the slightly different words of the OECD's Frascati Manual, "research comprises creative work undertaken on a systematic basis in order to increase the stock of knowledge, including man, culture and society" [p.15]. These deficiencies are limited by the way they take the basic processes of "investigation" and "creation" for granted. An investigation of the nature of creativity is beyond the scope of this thesis, but there is much of considerable sociological relevance that can be said about the general processes of investigation which, as it were, "contain" and express creativity. The work of Alfred Schutz with respect to this subject does in fact go further, I believe, than words taken for granted. The essence of his contribution is that research can be considered as a process occurring within "structures of relevance" - this gives *meaning*, in human terms, to processes that would otherwise remain essentially meaningless (that is to say, functional definitions such as those offered above give us no clues why anyone would bother to perform research - even if we regard motivation as irrelevant [which it is not] they do not make us any the wiser about the differences between research and the activities of machines).

Research is (as defined earlier) a process of creating and transforming objects of consciousness (or thought) by certain procedures in the context of "thematic", "interpretational" and "motivational" structures of relevancy [cf. Whitley, 1975]. In other words, research is a process of theoretical production. The meaning of research is then only comprehensible in terms of the context of its occurrence. What then are these various structures of relevancy mentioned above? Schutz's concepts of relevancy assume that the individual is most fundamentally concerned to make sense of the world - survival depends on this ability. If we can take this order producing faculty for granted (and given that this is the presupposition of all the social sciences, we are not being unusually naive) it follows that the individual has an "interest" in the surrounding world. This is true for scientist and non-scientist alike.

> "Interest determines which elements of both the ontological structure of the pre-given world and the actual stock of knowledge are *relevant* for the individual to define his situation thinkingly, actingly, emotionally, to find his way in it, and to come to terms with it. This form of relevancy will be called *"motivational relevancy"* because it is subjectively experienced as a motive for the definition of the situation". [Schutz, 1966:123].

What is interesting to the scientist will, to some extent, be determined by the kind of professional training s/he has had. The kinds of beliefs, values, theories, technique problems and goals that a scientist will more or less take for granted as part of a world s/he has been trained to function in will naturally vary from discipline to discipline, and specialised research area to specialised research area. At this stage in the argument it is not necessary to know precise details about the extent of this variability - perhaps in some respects there will be little difference between one scientist and the next. We will explore this aspect in more depth in the two case studies.

The interest that an individual has in the world is rarely uniform from moment to moment. Some objects and situations warrant more attention than others - in general, they are more or less useful to particular situations at hand. As well as this fairly routine variation in pragmatic utilities it may also happen that problems arise, as Schutz puts it,

> "It may happen that not all motivationally relevant elements foreknown in sufficient degrees of familiarity are adequate, or that the situation proves to be one which cannot be referred by synthesis of recognition to a previous situation typically alike, similar, etc. because it is radically new . . . Now the relevant is no longer given as unquestionable and has to be taken for granted: on the contrary, it is questionable but also worth questioning, and for that very reason it has acquired relevancy. That relevancy will be called *"thematic relevancy"* because the relevant element now becomes a theme for our knowing consciousness . . ." [ibid:124].

Just what situations are defined by the individual as typically alike will naturally depend on individual biography and particular patterns of socialisation. The identification of problems is usually sufficient to stimulate some kind of investigation which necessitates a sorting process:

> "The bulk of our fore knowledge is without bearing upon the theme and, therefore, immaterial for its being grasped and elaborated . . . [so] elements of the horizontally given stock of knowledge which are *interpretationally relevant* are brought to bear upon the solution of the thematic problem". [ibid:127, 128; my italics].

These are the essential features of the process of research in all universes of meaning - science and the everyday life world alike.

What has not yet been adequately explained is the institutionalised nature of this research, or in more general terms we have not yet made an explicit connection between the individual and institutional levels of analysis. So far we have described the individual scientist as confronting a pre-given world with established stocks of knowledge. In the normal course of events the scientist will also encounter problems in his day-to-day activities - these problems provide themes for research which then occurs essentially by a process of re-interpretation of previously taken for granted knowledge. All these processes are not, of course, occurring *in vacuuo*. The individual confronts a *pre-given* world and employs *pre-given* stocks of knowledge to deal with problems that may arise spontaneously, as it were, but which may well be also *pre-given*, in the sense that Kuhn defines "normal" science as a process of solving pre-given puzzles. These pre-given elements are for the scientist internalised in a lengthy process of adult socialisation whereby structures of relevance are established through a process of interaction between the scientist and his cognitive and social environment. So what appears for the scientist to be spontaneous behaviour does presuppose professional socialisation and the internalisation (a kind of "programming") of socially negotiated and traditionally based patterns of meaning. Motivational, thematic and interpretational relevance are socially mediated - authentic choices, as opposed to non-reflexive action, may arise as individual action but these are always in the context of symbolic interaction. As we will describe the situation in Chapter 4, the scientist inhabits a world of shared meanings which are sustained through a process of interaction with "others", viz, "professional orientational reference groups". The fact that science is highly institutionalised in general simply means that the degree of choise, or level of freedom that the individual scientist may have in his thought and action are already highly constrained by existing patterns.

Expressed in such general terms it should be apparent that the process of research is not something that only occurs in specifically research oriented occupations such as those of many scientists. Research, as such, is something that can in principle occur in all walks of life. It may happen, of course, that the social distribution of types of research is not uniform - this would be a fascinating problem for an empirical sociology of knowledge, but is beyond the scope of this thesis. What we will confront in this thesis are situations that have been defined by particular individuals as "scientific research"; I have attempted to map these particular processes of research in considerable detail. This "mapping" has been nothing more than the specification of various aspects of the types of relevancies defined above combined with a description of particular sequences of events which were historically significant to the outcomes of the various processes of research. While this type of description forms a central part of the work to follow, no research should stop at description; in this thesis the issue of relevancy will provide a theme for detailed investigation. In particular, why is it that criteria of relevance appear to vary for individuals depending on the nature of the social context; and how is it that particular research topics come to be considered as scientifically marginal - after all, the Frascati Manual [op.cit] may define research as something which increases the general stock of scientific knowledge but it is common knowledge that all research is not equal in the eyes of funders and publishers. In pursuit of these issues we have so far provided a general description of consciousness as influenced by structures of relevancy, which occur in a general context of professionalism. Science has been discussed in terms of the control of knowledge against "outside" interference, but we have yet to extend this analysis to the various sub-universes *within* the world of science. That is, how is it that professionalism controls the production, distribution and consumption of scientific knowledge within the world of science?

Professionalism within science necessarily involves the legitimation of particular sub-universes of meaning as more or less "marginal" to established scientific interests (the scientific "mainstream"). Any individual scientist's notions of relevancy are always, to some extent, mediated by the notions of relevancy of his peers and those elements of the profession which have intellectual and economic power and authority (or as discussed in Chapter 4, relevancy is mediated by "professional orientational reference groups"). There are furthermore, a number of indicators by which we can measure this "marginality". These indicators relate directly to the viability of the general process of integration, differentiation and reproduction of any social system and include the academic status of the research in the eyes of others, the general relationship of the research to industry and other potential "users" of research, the level and continuity of funding for the research and the level and continuity of recruitment procedures for the research.

Consequently, we cannot simply take an individual's notions of "interest" and "relevance" for granted as Schutz appears to do. In day to day behaviour in the "paramount reality" of everyday life the typical problems encountered by individuals which might give rise to small projects of research do not usually involve serious conflict over scarce resources. This is not the case in professionalised worlds such as that of modern science. Here the generation of specialised knowledge tends to be a highly capital intensive and consumptive process, and therefore the pursuit of interests becomes a much more politically contentious issue - who should do what, and with what level of support from public and private budgets? Thus the traditional phenomenological interest in the common stock of knowledge and everyday life does not provide much of a political or economic dimension that is necessary to explain the occurrence of particular structures of relevance in scientific consciousness. These remarks can of course be turned back upon Schutz's approach to everyday life and some more politically interested theorists have begun to do this. [18] This does not mean however, that Schutz's analysis, or Berger and Luckmann's general approach are to be discarded as simplistic or reactionary. Quite the contrary - concepts such as "universe of meaning" and "relevancy" may be partial understandings of the social world, but then again so is all social theorising. What is needed is more theorising, more synthesis and hopefully more and better understanding of the social world. What I am resisting however, is the assumption that the universal structures of consciousness which all phenomenologically interested researchers presumably pursue, do not have a political-economic dimension. We do need to transcend the partiality of Schutz's analysis.

One of the immediate benefits of the kind of synthesis that has been developed so far is that we are now in a much better position to begin to reassess the "realities" of scientific research which are presented in "official" statistics, elite influenced accounts of research, and the typical representations of individual practising scientists. But first we must continue to clarify our basic definitions of science.

2.7 Types of research: basic research versus practice oriented research

As discussed in the introduction to this chapter the existing typologies of research are not adequately defined in the literature.

Research was defined earlier (Section 2.5) as a process of theoretical production. Not all the products of this process are identical however, for the relevance of theory (or "knowledge" as defined in Footnote 2.6) may vary, on the basis of its orientation to production processes. All scientific theory can be situated (approximately) along a continuum of orientation towards application - in the terms of traditional usage "basic" scientific knowledge defines one extreme of least practical relevance and "technology" [19] defines an extreme of most practical relevance; "applied science" is then characterised by a type of knowledge which is not quite as practical or likely to be "embodied" in artefacts as "technology", but which is nonetheless centred on practically oriented objectives. Although these traditional terms have been used vary sparingly in this thesis, the distinction between different types of orientation in the products of research is quite fundamental, and forms the basis for a distinction between two types of research: basic research and practice oriented research.

Basic research is thus conducted primarily to acquire new knowledge without any particular application or use in view. [20] As the OECD's Frascati Manual points out though basic research may come to be quite closely associated with practical ends. As the Manual puts it, "applied research" can be "undertaken either to determine possible uses for the findings of basic research or to determine new methods or ways of achieving some specific or pre-determined objectives" [OECD, 1976:14]. In other words, basic research or "curiosity oriented" research as it is often called, may be relatively isolated from more socially oriented objectives, but all other research is directly linked to some socially relevant ends (hence "applied" research). This is of course a generalisation based on an apparently "obvious" logic, however as I go on to show in Chapter 3 and the case studies which follow, research may be influenced by a wide range of goals, and so just where one is able to draw the line between simple "curiosity orientation" and "applied research" may not be a simple matter - it may conceivably be the case that all research is conducted under the influence of some socially relevant goals. Nonetheless, what we can safely assume is that there will always be different degrees of social orientation in research. That is, research can be located along a continuum between basic, "curiosity oriented research" (as defined above) and "practice oriented research", [21] which is highly oriented towards social (/economic/political) ends. In other words, all research can be situated between two ideal types defining the extremes of social orientation.

As defined, practice oriented research is constrained by social and economic forces - e.g., to be relevant or economically viable; it is further constrained by the wider social and economic forces that shape the overall mission of the organisation which is supporting that research - e.g., by wider social expectations of a desirable new product, or its place in economic development and impact on man's ecology; practice oriented research is constrained (as is basic research) by intellectual or "cognitive" traditions which affect what it is appropriate to search for, and how to identify phenomena. But practice oriented research is distinguished from basic, "curiosity oriented" research not because of a lack of research goals in basic research or lack of social determinants in general, but primarily because of the latter's normally rather socially insulated location (e.g., in universities, well away from the front line of material production).

As this distinction stands there is still a considerable degree of arbitrariness associated with the criterion of "social orientation" - this is of course a natural consequence of the generality of the word "social". What is necessary for the sake of empirical research (as opposed to philosophising) is the specification of particular indicators which can be used to more readily compare different research programs. The following indicators have been explored in the case studies in this thesis: the degree of social orientation of the research goals held by scientists in a particular research program, the degree of orientation towards particular goals in the publications of a particular research group, the professional orientation of the "cognitive field" of particular scientists (see Chapter 4), and a general assessment of the difference between a "context of research" and a "context of legitimation" in a particular research program (including the degree of conflicting demands experienced by scientists - see Chapters 2.8 and 4), and lastly, scientists' own evaluations of their research.

The possibility of directing research to particular ends, or that is to say, the type of orientation possible in research depends on rather more than the mere will of particular scientists or bureaucratics, however. Thus, while it may be the case that most scientific research is noticeably oriented by socio-economic constraints and objectives, not all scientific resources (manpower, theory, techniques, instruments) are equally well developed for the immediate solution of problems. In the case of theory a certain minimum level of cognitive and social institutionalisation, or "maturity" of the parent disciplines is necessary, firstly for the unambiguous statement of problems, and secondly for the development of a solution "pathway". In a "mature" science or discipline (such as organic chemistry) much of the discipline's uncertainty has been ironed out: what is left are well argued frameworks and sets of problems, as well as methods of solving the central questions of the discipline. A mature science is usually seen to be one that is close to a point of crisis where scientists are likely to see less and less to add, so the potential for rewards is limited. It is a discipline that is perhaps moving into a stage immediately prior to its "revolution", or death. But a mature science offers relative certainty. If problems can be defined as soluble by the discipline, the chance of productive success is high. Thus, in basic research, mature sciences offer a high apparent productivity to specialists who can usually find a wide range of variations on a well known set of problems. But at the same time a maturely based specialisation situates the scientist more deeply in accepted and well reinforced orthodoxy. Thus, his/her creative energy is less likely to set the field ablaze with a new perspective that may be important for a progressive development of knowledge in the field to occur.

In practice oriented research, problems can often be defined into a mature scientific discipline - this is usually something of a pre-requisite in order that time and money invested towards the solution of a perhaps urgent problem is not wasted in unworkable results.

When a discipline or specialty is *immature* however, such as cancer research, [22] high atmosphere physics or sociology, the story is different. In such a case the cognitive field of researchers is characterised by a great deal of open-endedness and by uncertainty about which are the central problems of the discipline and which questions to ask of it; here the certainty of productive success is reduced. Even the formulation of the goals of research in this case may involve considerable uncertainty. These considerations have been explored in considerable empirical detail as differences in the levels of cognitive and social institutionalisation of research programs (see also Chapter 2.7). The theoretical basis for that empirical work will be further unfolded in the next section.

2.8 Cognitive and social institutionalisation

Whereas it is important to identify cognitive and social agglomerations such as disciplines, specialties and research programs as being units of organisation which preserve a sense of the wholeness of scientific reality, it is sociologically important to be able to make distinctions that cut across such units. The central concerns of the sociology of knowledge, for example, depend on a distinction between "knowledge" and the social context of knowledge production - just how knowledge reflects or incorporates its social milieu continues to remain *the* issue in the sociology of knowledge. This last issue is, indeed, of fundamental importance to an understanding of any distinctions that may be made between cognitive and social institutionalisation.

In this last respect, whilst it is conceded that the traditional sociology of knowledge has not been a field that has continued to provide sociology with rich fruit [see Adorno, 1978:452 for a particularly harsh criticism] and that it has still failed to solve its central problem, that of specifying the nexus between social and cognitive structures [see, for example, Merton, 1968:510], it is important to at least observe that some contemporary sociologists of science do in fact concern themselves with the same issue in the sociology of science - that is how scientific knowledge relates to its social milieu [see for example, Mullins, 1972; Whitley, 1974, and Edge and Mulkay, 1974]. But, whereas this is something of a novel approach in the sociology science it should be recognised that there are precedents in the sociology of knowledge. Further, although sound efforts have been made to establish scientific knowledge as quite amenable to a sociology of knowledge type of treatment [for example, Dolby, 1972; Bloor, 1973 and 1976; Barnes, 1977, and Mulkay, 1979] it is a contention of this thesis that just as the traditional sociology of knowledge often fails to descend from the clouds to useful case analyses, so too there is a danger that sociologists of science will go on and on talking about cognitive structures, the problem of ideology etc., etc., but fail to establish these concepts and problems as meaningful in the context of natural scientific knowledge. Consequently one of the main concerns of this thesis has been simply to make some theoretical and empirical sense of some of the concepts that are beginning to be bandied about in the sociology of science (for example, cognitive structure and cognitive institutionalisation).

It is however important that the general concerns of the sociology of knowledge continue to be at least addressed because unless attention is paid to the way in which cognitive and social structures (or factors, whatever) are shown to inter-relate there is another danger that the "new" cognitive orientation in the sociology of science [Blume, 1974:9ff] will become as cognitively one-sided as the traditional North American sociology of science was socially one-sided. What could be worse than an over-psychologised sociology?

Nonetheless we should not lose consciousness of the reputation of the traditional sociology of knowledge as "an idio-syncratic activity of sociologists with a penchant for the history of ideas" [Berger and Luckmann, 1963:69]. Consequently, in this thesis I have been fairly generally concerned with the subject of the institutionalisation of knowledge, a subject which falls in the ambit of what Berger and Luckmann [op.cit] have termed a "broader" sociology of knowledge "concerned with the whole area of the relationship of social structure and consciousness".

Although Martins [1974:252] has spoken of a "Cognitive Revolution" in various post-functionalist schools of sociological theory it would certainly be over-presumptive to assume that the revolution had begun in earnest in the sociology of science. It is certainly not clear from Blume's [1974:9ff] treatment what a cognitive approach even entails.[23] Nonetheless, in the beginnings that have been made speculation appears to be still strongly influenced by the cognitive idealism of the traditional approaches to science studies. Whereas cognitive structures may be highly significant as sources of authority [cf. King, 1971:3; and Bourdieu, 1975:19] we are still in a state of relative ignorance about the relationship between the cognitive and social dimensions of science.

In this thesis the problem of relating the cognitive and the social has been largely confronted through an analysis of research goals. The possibility of relating cognitive and social factors here derives from the fact that the goals of scientific research are constituted socially (broadly conceived) as part of a "cognitive field". Of course, a separation between the cognitive and social occurs in the process of analysis, but the concept of goal is in itself a dynamic one that is both social and cognitive in function at the same time. A goal is both part of the cognitive field of any individual and, when shared, the source of social action.

Two important concepts that have arisen in the more cognitively interested sociology of science are "cognitive institutionalisation" and "social institutionalisation". Whitley [1974:72] describes the terms as follows:

> "Cognitive institutionalisation has two major related aspects. First, it refers to the degree of consensus and clarity of formulation, criteria of problem relevance, definition and acceptability of solutions as well as the appropriate techniques and instrumentation. Second, it defines the activity of a scientist in terms of the consensus. In an area of

> relatively high institutionalisation we can predict with a fair degree of accuracy what a scientist will be doing, which models he will use and what sort of "ideal" explanations he will accept. Social institutionalisation also has two dimensions: first, the degree of internal organisation and boundary definitions, and second, the degree of integration into the prevailing social structures of legitimation and resource allocation".

It is implicit here that scientific research is actually constrained at all levels by structures which exist as a reality largely independent of the intentions of individual actors. The concept of institutionalisation used here refers to the patterning of actions and meanings, and is based on the existence of historically located and determined structures which predispose human action. Nonetheless, the notion of "structure" is still an abstraction from a reality that is usually experienced as continuous or "whole". Moreover, such structures are human products and as such open to change although this change may be opposed by the reification of structures as being beyond human interference.

Not all social and cognitive patterns are equally perceived as identifiable facticities, however, and so it is also important to be able to distinguish between levels of institutionalisation. Again, according to Whitley [1976:71] "level of institutionalisation" is "the degree of coherence and organisation of actions and perception and the extent to which ideas are articulated and adhered to". Unfortunately Whitley does not make any suggestions about how we might actually determine different levels of institutionalisation, and so we have here yet another important theoretical concept that is without an empirical dimension [cf. Whitley's units of organisation which were discussed earlier]. This notion of level of institutionalisation will be operationalised in the case studies which follow, but at this stage the concept can be made a little less abstract through a consideration of the process that appears somewhat antithetical to institutionalisation: "choice", and at the extreme, "chance". Clearly the higher the level of institutionalisation the less choices an individual is in fact able to make and the more likely we are to predict patterns of thought and action. Most people engaged in creative activity do of course struggle with the forces that would routinise their work - any situation is however, only different in degree so far as the escape from routinisation is possible. From this perspective level of institutionalisation is meaningful in terms of the level of choice available to individuals and also to the level of "chance" events possible in any given situation. In the case studies this "chance" aspect will be explored through the concept of "serendipity" - literally, the making of "happy accidents" [OED]. Serendipity is a function of the level of institutionalisation of any situation insofar as the more routinised and mechanical processes become the less likely is the intrusion of randomness into cognitive and social processes - randomness being, at root, the origin of serendipity.

These definitions are broadly useful, but they do need some further development before they can be empirically useful. As they will be used in this thesis cognitive and social institutionalisation implies structure. The social structure implicit above is quite obvious. With respect to a concrete research situation there are three main components in the social structure of a research program: organisations which provide economic and material support, the organisation of labour on a research program and thirdly, the relations of exchange and consumptions of the knowledge products of research (such as academic papers). This last component includes the social definitions of boundaries around areas of activity and knowledge. These components contribute to the functioning of a research program as a social system with mechanisms of differentiation, integration and reproduction, and also constitute for individual scientists a structured sub-universe of meaning. This much is fairly straightforward sociology. The structures associated with cognitive institutionalisation are not nearly so obvious however, and actually require some theoretical innovation.

2.8-1 The cognitive field of scientists in a research program

In this section I wish to change focus from a concern with the relatively objective status of shared meanings that characterise whole groups or classes of individuals (that is, social structures) to a concern with the perspective that any individual scientist will have as a typical conscious being. In this section I will explore the organisation of scientific consciousness within a particular sub-universe of meaning (the research program) a little more closely than I have before. A more theoretically precise treatment of many of the general principles on which this discussion is based will be presented in Chapter 4. At this stage we are concerned more with schematisation than fully elaborated theory.

Insofar as knowledge, beliefs, values, etc. (which I will refer to generally as "meanings"), are shared on a restricted basis amongst a particular group of scientists - which in a highly specialised scientific world is necessarily the case, we may safely assume that there is indeed a relative degree of closure in any sub-universe, and to that extent we can sensibly refer to a particular research program as if it were a relatively separable entity.

Any individual's consciousness can be regarded as capable of being constituted at any instant from a "relatively finite" stock of resources. That is, the range of an individual's consciousness is restricted by virtue of the particular structures of meaning that s/he has internalised during the ever continuing process of socialisation. This is a basic assumption of phenomenology, symbolic interactionism, and cognitive psychology, to name the most relevant sub-disciplinary approaches. In other words, an individual's thought is constrained by "cognitive structures". [24] These structures are not isolated entities inside the human head, however, since they are constituted and re-constituted in processes of communication, interaction and reflection. Cognitive structures will always have an individual *cachet* by virtue of the contribution of unique biographies to the formation of consciousnesses, but at the same time they are generalised by vurtue of forming any individual's sense of "generalised other". As Mead defines the "generalised other",

> "The physiological mechanism of the human individual's central nervous system makes it possible for him to take the attitudes of other individuals, and the attitudes of the organised social group of which he and they are members, towards himself . . . The very organisation of the self-conscious community [and individual, T.J.] is dependent upon individuals taking the attitude of other individuals. The development of this process . . . is dependent upon . . . getting what I have termed a "generalised other". [G.H. Mead in Parsons et.al. (eds.), 1965:739, 830].

What Mead doesn't stress, however, is that firstly this process involves the institutionalisation of knowledge into shared "stocks of knowledge" (as Schutz puts it) and that secondly, this process is not a simple one that ends at childhood, but one which may involve the formation of several relatively independent "others" which are "more" than "significant others" (that is, internalised particular individuals) but "less" than Mead's "generalised other" (that is, the mere consciousness of a self with some sense of continuity).

That is to say, the structures of meaning which are shared by individuals are always mediated by the presence of "reference groups" of a relatively anonymous nature. [25] In the case of scientists, consciousness is oriented by "professional" reference groups, which effectively control the broad contours of motivational, thematic, and interpretational relevancy. This is what it means to individual consciousness to say that science is professionalised (see also Chapter 4).

One of the presuppositions of Schutz and Berger and Luckmann is that consciousness is not a continuous phenomenon. Definite discontinuities in consciousness occur when individuals "leap" from one province of meaning into another (for example, in making transitions from "dreaming" to "every day reality" to the "research program", and so on. [See Schutz, 1974:24]. [26] The extent of these discontinuities may in fact be greater than commonly supposed, however. In the sub-universe of the research program for example, the empirical material gathered in this thesis suggests that the institutionalisation of a "context of research" as a relatively separated province from a "context of legitimation" may be an important discontinuity in scientific consciousness.

Whilst it is not possible to map the cognitive structures of individuals precisely (for reasons similar to Heisenberg's "Uncertainty Principle", namely that any observer is in a relationship to the observed, which restricts the amount of information that can be known about the observed) it is possible to obtain information about shared stocks of knowledge, shared goals, shared beliefs, and so on. These shared structures can of course only approximate to the structures in individual consciousness. With these remarks in mind the "cognitive field" of an individual scientist can now be specified more precisely.

The cognitive field [27] of scientists in a scientific research program is generally oriented by the forces of scientific professionalism through the agency of "profession orientational reference groups" (see Chapter 4) and exists potentially in (at least) two modes: a context of legitimation and a context of research. The context of legitimation emerges in situations where scientists are concerned with the justification of their research generally outside of an "in group" context of research. In the case of a relatively highly institutionalised context of legitimation, a coherent set of beliefs may exist as a cognitive structure which is entertained in relative isolation from other cognitive structures associated with a particular research program. The context of research on the other hand is dominated by research goals, and theoretical structures which provide structures of relevance which are of relatively immediate significance to the process of research. These two contexts will be further developed in Section 2.8-2 and 4.1. In summary, the cognitive field of any group of scientists is the field of possibilities and constraints which in conjunction with particular objects of consciousness, constitutes individual consciousness.

2.8-2 Cognitive structures in the context of research

As discussed in Section 2.6 research is a process which occurs in the context of various structures of relevancy: motivational, thematic and interpretational relevancy. The perception of relevancy by an individual is not usually a static process and so from an empirical point of view the best one can hope for are approximate structures which define the broad contours of relevancy for an individual.

The context of research, as defined above, is dominated by various cognitive structures which are continually mediated and oriented by various reference groups. These cognitive structures are conceived as an interpenetrating hierarchy of levels which range through a diffuse level of metaphysics, a theoretical level (a "theoretical landscape"), a level of subject concerns indicated by a "constellation of goals" and a technical level of procedures and techniques which are used during research. The idea that these structures actually exist in scientific consciousness is not new - Thomas Kuhn was perhaps the first to attempt the task of describing them through his notion of "paradigm"; much ink has been spilled over Kuhn's work and in the present context I have picked up on what "post-Kuhnian" refinements of the theoretical description of scientific consciousness exist in the literature rather than rehashing the very tired subject of Kuhnian paradigms (see, for example, Martins [1972] for an excellent critique of Kuhn's work).

As Whitley [1975:41] defines it, the metaphysical level of scientific cognition comprises "the overall system of values and beliefs which serves to justify and integrate the scientific activity with other systems of production . . . and provides a general world view". Those assumptions about the world which remain "tacit" [Polanyi] or deeply "sedimented" in consciousness [Schutz] or "themata" [Holton] belong at this level. For example, Holton [1974: 84] has identified pairs of dichotomous "themata" which have informed science over the ages: atomism/continuum, constancy/change, mathematical form/mechanical model, experience/symbolic form, etc. Included here are high level goals which need not be consciously associated with all phases of scientific work, but which may, nonetheless, provide a powerful directing influence on scientific research. For example, Mullins [1972:55] identified the high level goal of the "phage group" as determining "the secret of life".

The theoretical level, or theoretical landscape, includes scientific law, "standardised facts" [Ravetz, 1973], models and examples of theoretical application. Those elements of theoretical knowledge which become thematically and interpretationally relevant to research emerge from these horizons. But this level is not totally separate from other levels since any theory is only possible by virtue of metaphysical beliefs, goals and techniques which are sedimented into its being (perhaps at a very deep level) as an object which is meaningful in consciousness (see also Chapter 4). A typical scientific theoretical landscape is stratified into different levels: disciplinary and sub-disciplinary levels. These levels correspond with the various sub-universes of meaning that were defined in

Section 2.2 as the specialised stocks of knowledge that are shared at that level. It will be recalled that at the disciplinary level a particular way of ordering reality tends to be prescribed - for example, physics and mechanical engineering are understood as tending to entail different ways of seeing the world and different ways of going about research in the world. Disciplines are however, most effective as discriminatory social forces (as opposed to cognitive forces) since there may in practice be considerable overlap between different disciplines - as Whitley [1976:494] puts it, "Through their development as educational units of organisation, disciplines constitute the overall social and economic framework for scientific activity".

The sub-disciplinary level is an umbrella term covering the collectivities of specialty, research area, and research program, as defined in Section 2.2.

The level of subject concerns refers to the specific phenomena which are selected as legitimate areas of research. These subject concerns are necessarily indicated in the goals of research which provide specific directives for scientific action. Some of the goals of scientific action are more general than others, for example, a particular goal may be significant not only to a particular research program, but also across various sub-disciplinary concerns. In other words, research is always directed to a hierarchy of goals, or what I will refer to as a "constellation of goals". This structure of goals that gives any scientific universe meaning as a dynamic phenomenon is understood to define the level of subject concerns and, similar to the theoretical landscape, is typically stratified into disciplinary and sub-disciplinary levels.

There is an obvious interdependence here with the metaphysical and theoretical levels that have been defined above in that the specification of any phenomenon is a legitimate research concern will always involve theoretical and metaphysical (and "technical", as defined below) assumptions. For example, the subject of drug action in the brain as a specialty concern entails a set of theories about the action of drugs, and a set of metaphysical assumptions about the relationship between mind and brain and why one should interfere with either one of those "entities".

The technical level consists of the procedures, techniques, methods and instruments (as embodied technique) which are used during research in conjunction with elements from the metaphysical, theoretical and subject concern levels of the cognitive structure. This technical level is also not fully separable from any of the levels since scientific action (including research, speculation, communication and all the other human activities of scientists) is always dependent at some stage on technical knowledge which necessarily becomes sedimented into the products of research. In short, all of the cognitive structures defined above provide an interlocking structural context for research. The precise form and content that emerges as relevant in any particular research situation is necessarily a variable but this does not deny the inter-related "organic" nature of the overall field of scientific consciousness;

nor does it deny the possibility of discontinuities between sub-universes of meaning which incorporate these structures. The issue is always one of degree of separation (or inter-connection) rather than absolute divisions.

All of the cognitive structures described above are the historical products of processes of sharing of knowledge, beliefs, values, norms and goals in collective action towards shared goals. These structures have an objective and coercive facticity insofar as individual thought is constrained by virtue of being both a product of, and partner in, collectively based efforts to understand, explain and control the world. A cognitive structure has an objective status in that it is a description (albeit partial) of shared meanings, and is not reducible to private worlds. Such a structure is, to use Mannheim's expression, an "evidential construction", and would not necessarily be perceived (in toto) by the individuals whose accounts are used as the basis for a sociological synthesis - these structures derive from the extensive reciprocal typification of the world by scientists.

It must be stressed that these cognitive structures cannot be identically perceived by individual scientists. The situation is similar to the perception of one's physical environment. Neighbouring individuals in a street will almost certainly share the reality of a topographical map of the area, but those individuals will just as certainly perceive the street differently, if only for the reason that they do not all live in the one room. These differences are partially expressed through individual scientist's different priorities and different levels of involvement with the various goals that define a particular research program.

2.9 Summary and conclusions

The sociology of science has not so far provided a coherent theoretical framework which can encompass structure and meaning within the social and cognitive processes which constitute science. This chapter contains a structure of related concepts which provide both a coherent, theoretically based description of science and the basis for detailed empirical research ranging through microscopic analyses of day to day research to more abstract studies of the historical evolution of scientific knowledge.

Two broad themes have underlain the logic of development of this chapter - namely, that science and research are not unitary phenomena since they have meaning at different levels, and that a sociological perspective requires that the individual scientist be considered in the institutional context of scientific activity. These themes have been developed in a series of related concepts that will eventually permit a fully contextual analysis of two research programs.

In this chapter science has been described as both a universe of meaning and a system of theoretical production. These definitions are not mutually exclusive, despite the differences of their theoretical antecedents - phenomenology and structural-functionalism, respectively. However, if one is more concerned with the perspectives of the individual scientist and the constitution of scientific consciousness, science is, in the first instance, most usefully considered as a universe of meaning (in the sense used by Schutz, and Berger and Luckmann) - as opposed to a system of theoretical production. On the other hand, the fact that a universe of meaning involves the sharing of meaning forms the basis for an analysis of the more macroscopic aspects of science. This perspective requires more of a structuralist systems perspective. Nonetheless, both perspectives are necessary to an analysis which is reflexively aware of (and to some extent, integrates) subjective, objective and evidential levels of meaning (as defined by Mannheim).

The most obvious way to appreciate the relationship between structure and meaning in science would seem to be through an analysis of the kinds of forces which affect the day to day actions of scientists. Thoroughly done, this would require a diverse analysis with sociological and psychological dimensions - a task which would far exceed the scope of a single thesis. In this chapter I have focused however, on the professionalised nature of science - an understanding of this aspect of science being a necessary basis for any in depth studies of the scientific life world.

The relationship between individual scientists and shared structures of scientific and technical knowledge is effected through the agency of reference groups - this relationship is at all times mediated by scientific professionalism, which, as Terence Johnson uses the term, is a "collegiate" form of social control. The professionalised nature of scientific socialisation and research is particularly reflected in the concept of "professional orientational reference group" which will be developed in the next chapter.

Professional behaviour implies a range of attitudes and values (as Johnston and many others have discussed), but central to the attributes of professionalism is the value of autonomy. This value sustains the social fabric of science. In this chapter the internalisation of autonomy as a value has been described as a particularly efficient form of social control - the typical well socialised scientist is able to function as an effective professional without constant direct scrutiny from within the profession (as opposed to the "scrutiny" provided by internalised reference groups which effectively constitute a kind of scientific super-ego). Thus, when a scientist is acting as s/he chooses, in relative autonomy, the scientist is acting both as a professional and as s/he would "naturally" behave - that is, in good accord with the attitudes and values internalised during a typically lengthy period of adult socialisation; consequently, the system of science tends to remain self sustaining and relatively "maintenance free" [Berger and Luckmann].

Research in the sociology of science has tended to avoid the in depth consideration of laboratory life and the concrete processes of research. More specifically, researchers have to date not adequately reflected in their analyses the fact that scientific research is constituted through goal directed projects of action - a subject that will be further discussed in the next chapter. As intimated above, this failure has also partially derived from the apparent reluctance of researchers to give sufficient attention to that social collectivity most immediately relevant to the production of scientific knowledge - the research program. Whereas other collectivities (sub-universes of meaning/sub-systems) such as disciplines, specialties and research areas (as defined by Whitley) are significant scientific realities of different levels of generality, the research program is that sub-universe of meaning most immediately related to the production of scientific knowledge. The research program is the focus of creative synthesis for the individual scientist and will provide the focus of much of the empirical work contained in this thesis.

Scientific research is not a unitary phenomenon however. Different types of research do occur - this is primarily reflected in differences in the orientation towards application of research. Basic research and practice oriented research are distinguishable as two types of research defining different ends of a spectrum of application towards orientation - as reflected in the goals of the scientists producing the knowledge and the socially oriented practical utility that the knowledge is perceived by its users as having.

Scientific research is also distinguishable by its different levels of institutionalisation. As discussed in this chapter, the institutionalisation of all research has both cognitive and social aspects, and thus it is possible to distinguish between different levels of cognitive institutionalisation of research and different levels of social institutionalisation of research. This distinction between cognitive and social aspects is intended as an analytical tool which cuts across concepts such as finite province of meaning, sub-universe of meaning and system of production so as to facilitate a confrontation with the traditional concern of the sociology of knowledge with the relationship between knowledge and social structures. In this last respect the concept of research as goal directed projects of action provides a link between social structure and the knowledge produced by research. This link which has not so far been adequately conceptualised either in the sociology of knowledge or the sociology of science, would seem to be possible only through a deeper understanding of the way knowledge is produced as a consequence of goal directed social action.

The cognitive dimension of scientific research has also been largely neglected in the sociology of science - that is, science has tended to be conceived as a "black box". However, since the work of Kuhn a little more attention has been given to the constitution of the cognitive fields of scientists. In this chapter it has been confirmed that the cognitive fields of scientists tend to be highly structured (as originally suggested by Kuhn, Masterman, and others). Metaphysical, theoretical, subject concern and technical levels of structure of the cognitive fields of scientists have been distinguished theoretically as stratifications of "the context of research", a context in which scientists are primarily oriented towards research and the production of scientific knowledge. This context is not the only context which is significant as part of the sub-universe of meaning of the research program however, since modern-day scientists also necessarily engage in processes of legitimation of their research. To the extent that scientists do actually internalise a belief in the value freedom of research and the general irrelevance of social considerations as relevant to research one might expect a definite separation, in the consciousness and practices of scientists, between these two contexts. The extent of this separation will be explored empirically in the case studies which follow.

FOOTNOTES TO CHAPTER 2

1. In actual fact, this is a misquoting on my part since the continuum is defined as occurring between "immediately applicable research *and development*" and "highly abstract research *and development*". Assuming that research is different from development I have tidied up the situation by deleting "development" - this is consistent with ASTEC's actual usages of the terms. See also Hill and Jagtenberg [1977:18] where we make a similar suggestion about a continuum of application orientation.

2. This desired sense of movement between structure and meaning that has been outlined above has strong parallels in other areas of social theory. Lawrence Rosen [1971], for example, has written of the need for occupying a "phenomenological middle ground" in order that the possibility of drawing from diverse approaches remains possible. Rosen was particularly referring to the possibility of synthesising aspects of Sartre's work with that of Levi Strauss: one could begin by ascertaining through the dialectical methods outlined by Sartre in the Critique of Dialectical Reason the precise content of a particular group's perception of the "practico-inert" (that is, those things, persons, relationships and experiences that are perceived as inert, inhibiting and tangibly real). Then, turning to the structuralist techniques of Levi Strauss, one could try to show how these concepts become symbolically represented in such forms as myths and linguistic distinctions. In such a procedure one would be, according to Rosen, pursuing Levi Strauss' and Sartre's own interests in the nature of "mediating structures". The parallels here are illuminating. From a phenomenological middle ground of reflexive awareness of the various sub-universes of meaning (see Section 2.2) of scientist and sociologist there are "expressive meanings" generated through dialectically oriented interview techniques (see Chapter 5) on the one hand to be synthesised into structures of knowledge and beliefs on the other hand. Such parallels will not be pursued further in this thesis, but the broad issues will re-emerge throughout the work.

3. These sub-worlds are defined during secondary socialisation, which as Berger and Luckmann define it,

> ". . . is the internalisation of institution-based "sub worlds". Its extent and character are therefore determined by the complexity of the division of labour and the concomitant social distribution of knowledge". [Berger and Luckmann, 1973:158].

4. For detailed critiques of Kuhnian and Mertonian conceptualisations of the structure of science see Martins [1972] and Whitley [1972].

5. See Martins [1972] for what is still an excellent criticism of Kuhn's approach to scientific knowledge. There are of course similarities between the concept "finite province of meaning" [Berger and Luckmann] and paradigm [Kuhn]; in this respect Martins' criticisms of the concept of paradigm have some relevance as criticisms of the concept of finite province of meaning.

6. "Scientific knowledge" is defined here as those theories, or body of theories, which have been consensually accepted as "true" for the moment. This is a basically Popperian stance insofar as I understand the natural and social sciences as involved in an endless search for truths about the world. That is, we may approach the truth, by various criteria, but we may never be quite sure whether we have obtained ultimate knowledge. What is decidedly un-Popperian is the emphasis I give to social consensus rather than nature as the arbiter of truth. This stance is obviously contentious in its social relativism; "nature", I am asserting, is for the sciences more a matter of definition than we may be aware. We do all learn after all, is to see that "nature" which is in vogue. The fact that science "works", or knowledge is not refuted, is also a product of relative and partial perception. But such matters are not particularly relevant to this thesis. I state them only in order that my deepest prejudices be at least declared.

   What is particularly relevant to this thesis is the subjective experience of knowledge. For the purposes of this thesis it is sufficient to know that for a scientist "knowledge" is the certainty that particular phenomena are "real" and that they possess specific characteristics. As Berger and Luckmann [1973:13] define matters, "reality" is a quality appertaining to phenomena that we recognise as having a being independent of our own volition. Ultimately scientific phenomena and their characteristics come to be defined on the basis of a body of theories some of which may not ever be fully explicit. This situation means that scientific knowledge (and, indeed all knowledge) is "theory laden". Just which theories are present in the taken for granted research world of a scientist is a matter for empirical research, such as has been undertaken in the case studies of this thesis.

7. In this thesis I have used the concept of institutionalisation as a general way of referring to the various processes of sedimentation of meanings and actions into social forms. Institutionalisation is then the historical dimension of concepts such as "system" and "finite province of meaning" (as defined by Berger and Luckmann [1967] - this term will be discussed at greater length in subsequent sections). This is different to the usages of other writers - for example, van den Daele and Weingart [1975] distinguish cognitive factors (that is, "epistemological and intellectual factors") from "institutional" factors (that is, processes of differentiation, integration and differentiation). I prefer to retain a broader sense of institutionalisation since the whole problem of a "black box" sociology of science [Whitley, 1972] derives from the separation of cognitive aspects of science as being unrelated to the processes of institutionalisation - this is too harsh a criticism of van den Daele and Weingart's overall system, since they do not define cognitive and social factors as mutually exclusive, but in the light of their failure to emphasise the relationship, the general point remains. Nonetheless, in the interests of theoretical clarity I have related the indicators of different aspects of the institutionalisation of science used in this thesis to those systematised by van den Daele and Weingart [1975] in Table 5.8-1 in Ch. 5. Van den Daele and Weingart's scheme is in fact most useful as a preliminary theoretical guide.

8. Clearly, the concept of "profession" is more general in nature than particular professional organisations which are properly considered, only instances of professionalism. Furthermore, members of a profession may well be fully committed to being fully "professional" but may place little value on membership of their professional associations - thus in Australia, for example, the conservatism of the Australian Medical Association led some physicians to form the Doctors' Reform Society as a counter-professional group.

9. There is however, at least one occasion on which Johnson does appear to have the professionalised nature of science at the back of his mind:

> "We are, in part, engaged here in an analysis of professionalism as an ideology . . . The diagnostic relationship is given pre-eminence by those practitioners who personally confront laymen as an essential part of their work task and consequently need to have their expertise

taken for granted. While physicians and lawyers often find themselves in this situation, scientists rarely do".

10. In Australia, the major sources of funding are the government and private industry; the sectors of performance follow a different pattern however, with the Higher Education sector being, for example, a significant performer dependent largely on government funding. See Project Score - Research and Development in Australia, 1973-74, Canberra: Australia Government Publishing Service. Nonetheless, from both points of view - funder or performer - the general public is only indirectly involved. But more is involved here than just physical or intellectual distance, for communication between scientist and user is mediated by a potentially large number of institutions, each of which may have its own goals.

11. The recent [1977] Royal Commission into Uranium mining in Australia provides example of the way the general public are effectively screened out of critical debates. Whilst the Ranger Enquiry was theoretically "open" to public contribution, one would have needed to be a particularly devoted and knowledgeable lay person to penetrate the inner sanctum of professional scientists, administrators and industrialists.

12. This latter situation of negotiation between "in house" scientists is not evidence for science having a form of "mediative control" [Johnson, op.cit:Ch. 6] since an institutionally separate third party (such as the state) does not usually intervene between producer and consumer - as occurs in the case of welfare work and some form of legal work, for example.

13. The amount of useful empirical information about the value of autonomy to scientists is very limited. Apart from the information provided by Hill et.al. which we will discuss in this section, information about the values of scientists has tended to be highly influenced by Merton's ideas about the norms of science, which have remained relatively unquestioned. Cotgrove and Box [1970] for example, used questionnaires to determine the importance placed by chemistry students and industrial chemists on autonomy. The results appeared to indicate that not all scientists valued autonomy equally - the results showed some differences amongst "public", "private" and "organisational" scientists but as the researchers pointed out, questionnaires are "somewhat crude measuring instruments" and furthermore, there was doubt about the validity of the

question on autonomy [ibid:32, 33]. In the light of the case studies in this thesis any approach which did not seriously question scientists' own opinions - via an analysis of scientists' deeds as well as their words, must be held suspect.

14. Results of this survey of Australian scientists are presented in detail in S.C. Hill, et.al. [1974].

15. See, for example, Michael Polanyi, The Contempt of Freedom, London, 1940.

    Polanyi's arguments are best understood in the light of their origins for they are essentially politically inspired - Polanyi was, in the thirties, instrumental in organising a Society for Freedom in Science, not simply as an expression of a "good" in itself, but as the organised opposition to the Association of Scientific Workers, which represented early attempts to unionise scientists in Britain. Polanyi was most concerned about scientific activists (including many highly respected scientists such as Bernal, Needham, Blackett and Haldane) should not upset scientific orthodoxy with their criticisms of scientific abuses, middle class interests and authoritarian elitist organisational practices in the science of the time.

16. A vitriolic controversy flared amongst earth and astronomical scientists with the publication of Emmanuel Velikovsky's theories and evidence of planetary chaos in the early 1950's. For reviews of this case, see A. DeGrazia, The Velikovsky Affair, London, 1966. The significance of this case in relation to the profession of science is drawn out in Stephen C. Hill, "Engineers and Environmental Action", Journal, Institution of Engineers, Australia, 45 [September, 1973], (9), pp.25-28.

17. This observation is reported in Stephen C. Hill, "Some Problems in Research Management", Proceedings, Royal Australian Chemical Institute, July 1968, pp.173-177.

18. See, for example, Barry Smart's Sociology, Phenomenology and Marxian Analysis, London: Routledge and Kegan Paul, 1976. This treatment does appear to reflect an unsympathetic and, at times, superficial appreciation of the phenomenological tradition, however.

19. Taken here quite literally as knowledge (logus) of the arts (techne), conventionally meaning the practical productive arts. For a further discussion of the concept of technology see R.M. Bell and S.C. Hill [1977].

20. cf. the definition offered in the OECD's Frascati Manual:

> "basic research is conducted primarily to acquire new knowledge of the underlying forms of phenomena and observable facts, without any particular application or use in view" [1976:13].

The assumptions about "underlying forms" and "observable facts" are not necessary (even if true) to our definition given the perspective on scientific knowledge outlined in Section 2.3.

21. In other work I have used the concept of "mission-orientation" in the same sense [Jagtenberg, 1975; Hill and Jagtenberg, 1977; and Johnston and Jagtenberg, 1978]. The term "mission orientation" first appeared in science policy and survey material, for example, Byatt and Cohen's submission to the U.K. government concerning the economic returns of basic research, "Science Policy Studies No. 4: An Attempt to Quantify the Economic Benefits of Scientific Research", London: HMSO, 1969, and the Frascati Manual, OECD, 1976. Subsequent to this the term appears to have diffused into more academic circles - see, for example, Bohme et.al. [1976] and Jagtenberg [1975]. I have preferred the term "practice orientation" here for reasons of theoretical clarity. All scientists do have, after all, some sense of mission and the important distinction between types of research is not so much on the basis of the sense of mission but on the kind of mission that characterises research.

22. Cancer research is a typical example of problem oriented research where the basic disciplines involved are insufficiently mature for progress to be made without major "basic" research occurring at "frontiers" of knowledge in molecular and cell biology.

23. Given the relative novelty of a cognitive approach in the sociology of science, other specialties in which a "cognitive revolution" has been foreshadowed (at least) might well provide valuable insights for the aspiring "cognitive" sociologist of science. For example, Roland Robertson's remarks in the context of the lack of cognitive considerations in the sociology of religion are directly relevant to the sociology of science:

> "The sociology of religion has tended during the past few decades to ignore the cognitive side of religious commitment - although a few sociologists have attempted to inject cognitive factors into the sub-discipline . . . where beliefs have been relatively unexplored. Even more disconcerting, the kinds of religious beliefs which have been tapped have almost invariably been of the surface, institutional kind. That is, sociologists interested in beliefs have typically limited their analyses to the "official" beliefs manifested in organizationally-promoted settings". [Robertson, Meaning and Change, Oxford: Blackwell, 1978:232].

These remarks will be additionally salutary if they curb similar tendencies in "post-functionalist" sociologists of science!

24. This term often arises in cognitive psychological discourse. For example, as the authors of a popular introductory psychology text put it,

> "Those identified with the cognitive viewpoint argue that learning, particularly in humans, cannot be satisfactorily explained in terms of stimulus-response associations. They propose that the learner forms a *cognitive structure* in memory, which preserves and organises information about the various events in a learning situation". [E.R. Hilgard, R.C. Atkinson and R.L. Atkinson, Introduction to Psychology, New York: Harcourt Brace Jovanovich Inc., 1975:215].

Whilst this thesis is not an exercise in cognitive psychology as defined by cognitive psychologists it is of some interest to note that there is an obvious overlap of interest between this thesis and the general interests of cognitive psychology. This is true insofar as there is a mutual concern with "concepts of the type [which] may be descriptively labelled as "cognitive" because they assign a prominant role to man's planful intellective processes [sic] in the guidance of his behaviour" [M. Manus, An Introduction to Cognitive Psychology, Sydney: Prentice Hall, 1973:vii].

25. See R.K. Merton [1968:Ch. 10 and 11] for a history of the concept of reference group. The sense in which I am using the term will be elaborated in Chapter 4.

26. The analysis of these "leaps" in consciousness is not particularly convincing in the work of Schutz, Berger or Luckmann. At base there simply appears to be the relatively naive assumptions that these discontinuities just occur as a necessary concomitant of the commitment to the existence of discrete finite provinces of meaning. The reason(s) why they occur could stand more consideration. One hypothesis that is worth exploring further (but which is beyond the scope of this thesis) is that discontinuities in consciousness are directly related to the existence of relatively separate and perhaps conflicting orientational reference groups which mediate an individual's stocks of knowledge and notions of relevancy. That is, there needs to be more attention focussed on the process of identity consciousness through "orientation of others". See also Chapter 4.

27. The recurrent use of geographical and geological metaphors in this thesis has been largely inspired by Schutz's theoretical language (for example, *horizons* of meaning, *provinces* of meaning, *sedimentations* of knowledge, *contours* of relevance). More generally speaking the theoretical language I have employed is resonant with the visual orientation of any sociology that attempts to deal with *structures*.

CHAPTER 3: THE IMAGE OF A DIRECTED SCIENCE

". . . the premises of science cannot be explicitly formulated, and can be found authentically manifested only in the practice of science . . ."

M. Polanyi, Science, Faith and Society, Chicago: The University of Chicago Press, 1973, p.85.

3.1 Introduction

Now that a broad theoretical framework for the analysis of the physical sciences has been established it is possible to focus more narrowly on specific critical issues that have arisen in the course of the development of the definitions in Chapter 2. This present chapter is particularly important in that it establishes an empirical basis for some of the themes that will be developed in subsequent chapters.

One of the most central concepts developed in Chapter 2 was the idea that scientific research is a goal directed process which occurs within the context of structures of relevance. In this chapter we will begin to examine this concept more closely and show that some of the prevailing assumptions about the nature of particular scientific goals are problematic.

This apparently narrow focus on the goals of scientists does however derive from a number of broader issues that were raised in Chapter 2. Most significantly though, the "objectivism" of many of the prevailing images of science tends to be derived from the naive acceptance of mass survey data. As we have discussed, science and research are not unitary phenomena of a merely "external", "objective" nature. Nonetheless, the naive assumption that they are has continued to persist in discussions of the social relevance of science - this assumption is, as we will show in this chapter, consistent with the image of science presented by mass survey data. Mass surveys tend, moreover, to be insidious in their effect, for even if sociologists are wise to their limitations they often tend to be the only source of extensive data about particular phenomena. Almost invariably then mass survey data tend to be incorporated at some level into sociological analyses, carrying with them the limitations of their basic assumptions. Studies of science tend to be no exception and given the potential relevance of mass survey data to this thesis, the present chapter partly serves as an exploration of the limitations of survey data in an Australian context.

More broadly considered, this chapter begins to explore issues related to the possibility of social relevance in science. To re-iterate one of the central concepts involved here, research is usefully considered as a type of goal directed social action that is performed within the context of institutionally defined structures of relevance. The questions that we will discuss in this chapter are central to an understanding of the relevance of goal directed actions of scientists since these questions are primarily concerned with some of the images of social relevance that have been at least partially conspired in by scientists. The image of a directed science, to mention the central image, has not however been publicly questioned by sociologists or physical scientists - presumably it is still politically expedient to believe in the existence of a science that is becoming increasingly directed towards social relevance. Whether or not such an image is true is not the primary concern at this point however. The major issue which this chapter begins to explore concerns the problem of how we can discover the actual objectives of research as opposed to naïve assumption and the simplistic implications of mass survey data.

As we will see, the image of directed science must be considered in the context of scientific professionalism and scientists' desires for the autonomy of science (subjects which were introduced in the last chapter). Only in this light can we begin to untangle the apparent contradictions of scientific practice.

We start the investigation of this chapter with an examination of Australian science on the basis of existing data and its implications.

3.2 Images of Australian natural sciences according to Project Score

On the basis of Project Score data over the nine year period 1968-1977 [1] I have attempted to contrast overall expenditure on natural scientific research in Australia against the objectives to which this research is ostensively related. [2]

The results suggest that Australian research is highly industrially penetrated and socially constrained. When Australian R & D effort is classified by its socio-economic objectives, it is clear that research directed towards "economic goals" accounts for the major part of available funds. From Table 3.1, the priorities in expenditure in 1973-74 were (in decreasing order) "business", "economic development", "advancement of knowledge", "national security", "community welfare", and "international welfare". It may be safely presumed that this order of priorities had not substantially changed over the next three years

TABLE 3.1: Summary of R & D expenditure within Australia in the natural sciences - by broad socio-economic objectives and "type of activity" [a].

SOCIO-ECONOMIC OBJECTIVES	EXPENDITURES [b]					
	1968-69 $M	%	1973-74 $M	%	1976-77 $M	%
Economic Development (primary industry [c] secondary industry [d] economic services [e])	105.0	34.3	149.3	25.2	289.9	
Business [f]	84.8	27.7	228.0	38.2	-	
National Security [g]	61.9	20.2	56.0	9.4	87.6	
Advancement of Knowledge [h]	29.0 (70.9)	9.5 (23.2)	129.2	21.7	128.6	
Community Welfare (health, environment, public welfare, community services)	25.5	8.3	31.9	5.4	63.4	
International Welfare [i]	0.1	0	0	0	0	
TOTAL	306.3	100	594.4	100	569.6	
TYPE OF ACTIVITY						
Basic	58.1 [j]	26	129.6 [k] (151.7)	35 (25.5)	200.3	35.5
Applied	100.9	46	171.6 (221.0)	47 (37.2)	273.0	47.9
Development	62.5	28	65.1 (221.6)	18 (37.3)	96.2	16.6
TOTAL	221.5	100	366.4 (594.4)	100 (100)	569.6	100

TABLE 3.1 (cont.)

Source: Department of Science Project Score - Research and Development in Australia, 1968-69, Canberra: Australian Government Publishing Service, 1973; and Department of Science, Project Score - Research and Development in Australia, 1973-74, Volume 2, Canberra: Australian Government Publishing Service, 1976.

[a] "Socio-economic objectives" were used in the last two SCORE surveys, as were "types of activity". Both terms are used in the SCORE reports; however, their meanings are not well defined.

[b] At current prices. Minor discrepancies in totals are due to rounding.

[c] In the 1968-69 SCORE this was "agriculture".

[d] In the 1968-69 SCORE this was "industry".

[e] In the 1968-69 SCORE the figure excludes "civil nuclear" and "civil space" categories which were grouped under "national security and big science".

[f] I would suggest that the category "business" should be included under the objective "economic development"; however, "business" is listed separately in this analysis because no breakdown of socio-economic objectives for that sector is available in the SCORE Reports. Both the 1968-69 and 1973-74 Business Enterprise surveys use a classification of type of activity based on the Australian Standard Industry Classification. In the 1968-69 survey attention was confined to the mining and manufacturing industries, whereas in the 1973-74 survey an attempt was made to estimate R & D across all industries. Thus the figures presented here are not strictly comparable. The only potentially comparable figures are for manufacturing and here reference to the detailed statistics will show an increase in R & D expenditure of approximately 125% (at current prices) over the five year period.

[g] The 1973-74 figure is just "defence" expenditure, as opposed to the 1968-69 figure which includes "civil nuclear" and "civil space" expenditures under the objective "national security and big science".

Moyal [1979:77] has pointed out that the 1973-74 figure has been challenged as covering only the allocation to the R & D Division of the then Department of Supply. "It excludes funds allocated to other activities, such as Production, Development and Navy, Army and Air Laboratories and Establishments, which remain hidden in departmental totals. These extra allocations increase the total figure to $76 million".

[h] The bracketed figures are my adjustments of the 1968-69 figures so that a comparison with the 1973-74 figure for "the advancement of knowledge" objective (*only*) can be made: all R & D expenditure in the higher education sector has here been reclassified as "towards the advancement of knowledge".

[i] The figure given for 1968-69 was then classified as being towards "developing countries".

[j] For 1968-69 this excludes business sector data which are not available. The bracketed figures on the 1973-74 details include business sector data.

[k] The bracketed figures in the 1973-74 details *include* business sector data.

to 1977. A more detailed breakdown of these figures is presented in Table 3.2. No major changes in priorities appear to have occurred over the nine year period.

Although basic research accounted for 26% of natural science research expenditures in 1968-69, and 35% in 1973-74 and 1976-77 [3] (all three figures do not include research in the Business Sector) it should not be presumed that this apparently high priority necessarily means that there is a great deal of research that is simply concerned with knowledge "for its own sake". These figures are misleading because basic research is defined as "original investigation" where the primary aim of the investigator is a more complete knowledge and understanding of the subject under study. As other data collected in Project Score suggests, this primary aim is often directed towards some broad field of socio-economic interest - such as agricultural productivity, chemical technology or electronics. That is, "knowledge for the sake of knowledge" can also be shown as often occurring within a socio-economic "relevance structure".

In fact, inspection of the data in Table 3.2 shows that while in 1968 expenditures only 9.5% of research was directed towards the "advancement of knowledge", [4] 19.3% of research was "basic research" (including research in the business sector on the basis that none of it was basic research). Thus, to use the terminology of the Frascati Manual 51% of "basic research" was actually "oriented basic research", or research "oriented or directed towards some broad fields of general interest". [5]

In 1973-74 similar evidence for the social orientation of some basic research emerges, but the proportion is smaller (14.5%) and on

TABLE 3.2: Aggregate [a] R & D expenditure within Australia in the natural sciences - by socio-economic objectives [b].

SOCIO-ECONOMIC OBJECTIVES	EXPENDITURES 1968-69 $M	1968-69 %	1968-69 Rank	1973-74 $M	1973-74 %	1973-74 Rank	1976-77 $M	1976-77 Rank
Defence	51.2	16.7	3	56.0	9.4	4	87.6	4
Primary Industry	56.5	18.5	2	83.6	14.1	3	169.2	2
Secondary Industry	29.2	9.5	5	31.3	5.3	6	48.7	6
Business	84.8	27.7	1	228.0	38.4	1	-	1 [c]
Economic Services	30.0	9.7	4	34.4	5.8	5	72.1	5
Health	17.6	5.8	7	11.5	1.9	8	40.7	7
Environment	5.4	1.8	8	13.1	2.2	7	18.5	8
Public Welfare	0.7	0.2	10	0.3	0.1	10	0.7	10
Community Services	1.8	0.6	9	7.0	1.2	9	2.3	9
Advancement of Knowledge	29.0 (70.9)	9.5 (23.2)	6 (2)	129.2	21.7	2	128.6	3
International Welfare	0.06	0	11	0	0	11	0	11
TOTAL	306.3	100		594.4	100		569.6	

Source: Department of Science, Project Score - Research and Development in Australia, 1968-69, Canberra: Australian Government Publishing Service, 1973; and Department of Science, Project Score - Research and Development in Australia, 1973-74, Volume 2, Canberra: Australian Government Publishing Service, 1976.

[a] That is, expenditure across all sectors of performance.
[b] Refer to notes for Table 3.1.
[c] Although the figure is not available it is highly unlikely that a major shift in priorities occurred over the three year period.

a different basis (some basic research can be inferred as occurring in the business sector). The data at hand for the period 1976-77 is still incomplete and so further comparisons cannot be drawn.

One interesting observation can be made from 1973-74 data, however: if research is categorised as "pure", "applied" or "basic" such that the business sector data are included (the bracketed figures for 1973-74 in Table 3.1) the proportion of research that is categorised as basic is considerably reduced (by 10%). This is clearly because a larger proportion of R & D in the business sector is actually "development". In other words, the exclusion of business sector data leads to a distorted picture of the overall pattern of research in Australia.

There are other serious objectives that can be raised against the image presented by the data. These are related to the difficulty that a researcher may have in relating his/her research to general categories, and secondly to the possibility of the spurious labelling of research for the sake of political and economic expediency. Thus, in the first case it may well be that the researcher who is either unaware of the social context or consequences of his/her research, or who cannot quite fit himself with any of the applied objectives supplied by Project Score, simply decides on "basic research" as a convenient label for his work. In other words, such a categorisation may not be accurate and "knowledge for its own sake" may sometimes be highly socio-economically oriented despite the beliefs of individual researchers. And, of course, vice versa, it may also be that an individual scientist's categorisation of his/her research as socially useful is equally unrealistic. In the second case, that of labelling for the sake of expediency, it may be that in the present economic climate the socio-economic relevance of research may be exaggerated by scientists in order to gain favour with the more pragmatically oriented funders of research. It is likely that such exaggeration would be carried through to survey responses if only for the sake of consistency (which is, after all, one of the hallmarks of good science). All this is, unfortunately, largely speculation at this stage, but until we have more detailed information about the research practices of Australian scientists (and indeed any other nationality of research) we would be well advised to use survey data more cautiously.

In the absence of comparable data for other advanced western economies, I am not suggesting that this Australian data is typical in all respects; what I am suggesting, though, is that the kinds of socio-economic objectives that have been used are typical of the level of abstraction that is often used as the basis for understanding what the overall directions of the sciences are. Subsequent chapters of this thesis will show how this level of abstraction may well serve to obscure the way that research is actually directed in the process of research.

3.3 The inertia of the Australian physical sciences

Despite the limitations of the SCORE surveys (see particularly Footnote 2 in this chapter) there are a number of further observations that can be made about the pattern of R & D expenditures as uncovered by the three surveys. One is that expenditure on defence research was only slightly higher in 1973-74 than it was 5 years before but rose considerably in the period 1976-77; the second is that while expenditures on "community welfare" rose by 25% in that period, the actual change of expenditure was only $6.4 million: the overall percentage of all research moneys directed towards community welfare appeared to drop rather than increase.

Between the periods 1968-69 and 1973-74, the Liberal Government whose policy was "to have no policy on science" [6] was replaced by a Labor Government which had an established policy of changing the orientation of science towards a "science for the people". The Labor Government came to power on a platform of concern with public welfare and the environment, a platform which was supported by, and which in turn supported, a general raising of public consciousness in these issues. It appears that they had some success in constraining the expansion of expenditure on defence research, but this could have been achieved relatively simply by merely curtailing expenditure to one arm of the government services. On the other hand, despite overt policy changes, it appears that change towards a science "for the people" - expressed in "community welfare" expenditures - was of little consequence. Further, following our earlier hypothesis about labelling for the sake of expediency, what appears to be change in expenditures may not be so when one realises that in responses to the Project SCORE questionnaires in 1973-74 the respondents could easily have reclassified work they had been conducting in 1968-69 into new categories that expressed new expectations (e.g., ecology research), though their research changed very little. Meanwhile, real changes in research expenditure that may have occurred and which may have been something of an attempt to generate a "science for the people", appear to have been severely constrained by a pervasive intertia on the part of scientists, and by political battles which resulted from the Labor Government attempting to introduce these changes. For example, the $1 million Botany Bay project - an interdisciplinary study of a whole industrialising area and its people - appeared to fail because (a) scientists generally conducted research on what they were interested in rather than on what was most relevent to the whole project, and (b) the project generated a bitter State-Federal political clash that finally brought about the project's downfall.

The major changes over the period 1976-77 (which was marked by the removal of the Labor Party from office) were changes in the pattern of expenditures towards primary industry, health, public welfare and the advancement of knowledge. Most significant was the actual decrease in expenditure towards community services and the advancement of knowledge, both of which objectives were regarded by the Liberals as extravagantly dealt with in the Labor budget. But overall the general pattern of research priorities tended to remain very much the same. As one Project SCORE researcher commented in a private communication - "governments may come and go, but research objectives stay much the same".

These observations, limited as they are, suggest a fair degree of inertia to real changes of direction in scientific research. At least part of this inertia lies in the autonomy scientists retain to continue doing research on the specialised interests for which they have a trained competence. Other evidence suggests that the earliest selection of areas of specialisation is more ruled by the specialised competences of academic supervisors than by changes in government objectives, community consciousness of issues, or even by job prospects for graduates. [7] For example, within the discipline of chemistry, the highest projected "industrial demand" for Ph.D graduates, in 1964, [8] was for analytic and high temperature chemistry, while the actual production of these graduates from 1964 to 1972 rose only from 0.7% to 1.4% of all chemistry graduates; meanwhile, in 1964 co-ordination chemistry and theoretical chemistry/spectroscopy had the *lowest* projected industrial demand, but the proportion of graduates in these specialties had substantially increased by 1972 from 12.4% to 22.6%, and from 7.2% to 14.9% respectively. Most university science disciplines (with perhaps the notable exception of organic chemistry) have continued to develop, and produce Ph.D graduates in specialties that are already there; University science training has reacted very little to the movement in potential employability of research graduates they produce. [9]

The most fundamental reason, perhaps, for this insensitivity of academic science training to influence from the wider community (via job prospects for graduates) rests with the *autonomy* of acacemic scientists and their need to further career goals through the work conducted by their research students who are thus trained in precisely the same disciplines as whichever supervisors are around. Other evidence suggests that intending students react very little to job prospects in choosing to do a Ph.D (or in 'not choosing to do one', since most of the students surveyed were *channelled* into doing a Ph.D).[10] So once a discipline specialty is established in a university, research student numbers will continue to grow depending on the popularity of the supervisors, and the number of research studentships which are around. Ph.D graduates tend to remain in that same area of research after they graduate as they have little alternative professional capability anyway. It is interesting that one product of the specialisation that occurs within the relative *autonomy* of university scientists is a demonstrable *non adaptability* in graduate scientists. Because the autonomy of university supervisors is only weakly mediated by the wider community there is very little incentive to change established patterns in scientific training unless, of course, such pressures should come from the professional community itself. But, consistent with our early discussions of professionalism, the profession of science values autonomy and, furthermore, a profession by its nature is far more likely to be conservative than the wider community - that is, professional values are less likely to undergo change than community values.

3.4 The image of a directed science

The Australian data we have discussed clearly supports the claim made in Chapter 1 that contemporary sciences often give the appearance of becoming increasingly directed towards socially useful ends. As suggested in Chapter 1, the world wide concentration of research in industrial and government laboratories has made the question of the accountability of research an increasingly dominant issue amongst scientists and all those with social, political and economic interests in the outcomes of research. This would appear to be reflected in the Australian data where less than 10% of all research could possibly be considered in relative isolation from socio-economic categories. Furthermore, as the data presented in this chapter suggests, the percentage of research that could possibly be considered in relative isolation from socio-economic categories is probably less than the category "basic" research may suggest.

This image of a directed science is not completely unambiguous, however. In spite of the considerable evidence that research may be rightly regarded as a highly controlled and directed activity, academic scientists and in particular the elite who represent the scientific community in negotiation with funding agencies and political interests, still continue at times to project an apparently contrary image of science as autonomous and apolitical, concerned solely with the objective pursuit of truth, i.e. as "pure academic science". In other words, we have a situation where scientists on the one hand have clearly collaborated in the generation of an image of sciences that are socially relevant by virtue of being oriented towards particular socio-economic goals, and yet scientists on the other hand continue to project an image of "pure academic" science as representing all that is best in science. Later in this chapter this ambiguity will be discussed in terms of a basic ambivalence in the sub-universe of the research program, but first we need to make a more convincing case for the assertion that scientists still relate strongly to an image of science as being ideally "pure" and autonomous.

The reaction to the most recent government enquiries into the organisation of scientists in the United Kingdom and Australia demonstrates this point quite clearly. The strongest defensive reaction came from the British scientists who, unlike their Australian colleagues, were eventually reorganised, despite their protestations, under the terms of the Rothschild Report. [11]

Prior to 1970 the "Haldane Principle" tended to remain as an important precedent for British science policy makers. Under this principle scientists were guaranteed a relatively independent relationship with politicians - Research Councils were independent of Departments of State which might be expected to be affected by their research. As Rose and Rose [1976b:23] point out though, the level of autonomy actually experienced by British scientists began to be eroded after 1945.

> "From 1945 onwards, successive governments (whether Labour or Conservative) drew the net of state-science interaction tighter, culminating when, under the 1970 Conservative government, the Rothschild Report, A Framework for Government Research and Development, challenged the Haldane principle head on,

> and, over the vociferous protests of the scientific elite, was accepted as the future basis for the management of science".

The Rothschild Report is in fact, a landmark in the history of science for it quite clearly indicates the kind of relationship which is particularly appropriate for an increasingly instrumental society. In the very words of instrumental rationality:

> "This report is based on the principle that applied r and d that is r and d with a practical application as its objective, must be done on a customer-contractor basis. The customer says what he wants, the contractor does it (if he can): and the customer pays".

Clearly, this attitude makes science the equivalent of material systems of production whose products can be "freely" exchanged in the economic market place. A comparable Australian example is provided by the more recent response of elite scientists and administrators to suggestions from government that the CSIRO might benefit from reorganisation aimed at increased accountability. The Philip Report [12] was produced as an essentially political response aimed at defending the autonomy of science from what was perceived as primarily a threat from "outsider" bureaucrats. It is interesting that the Philip Report used as its basis the now rather outmoded Mertonian concept of a social system of science, which as it happens reinforces the notion that autonomy cannot be neglected lest both the quality of science and the productivity of scientists suffer: [13]

> "Creative productive science depends on the autonomous operation of self-imposed values and controls. It is ultimately self-defeating for a society or government to erode the autonomy of the scientific community. [Philip, et.al., 1975].

Both these examples clearly demonstrate that modern science has an ideological aspect. [14] The "pure academic science" image which is centred on the professionally derived value of autonomy can be rightly considered as a modern ideology aimed at protecting scientists from external control:

> "The theory that scientists follow only the internal rules of science would seem to reinforce their efforts to prevent the subordination of their work to standards extrinsic to science and to protect themselves from external political influence". [15]

In the light of the apparently high level of socio-economic orientation of most science (as we have discussed) this image would seem to involve something of a distorted picture of the reality of a science which is already highly influenced by "external" criteria of social relevance.

The extent to which this protective self-image has been incorporated in the study of science and its institutions has been shown by Johnston [1976] to be due to the pervasiveness of the analytical dichotomy between 'internalist' and 'externalist' explanations which reinforces the notion of science as a social system separated from, but occasionally influenced by, other social forces. It is also evident in the concentration of studies on precisely that small sector which most nearly approximates this model - that of academic, university-based, 'pure' research. [16]

The idea of "pure academic science" is not, of course, the only image that is associated with science. In the face of the often obvious direct connections between scientific research and applications of that research in industrial and military contexts it would be counterproductive from an economic and political point of view for scientists to maintain the image of a "pure academic science" as the only type of science worth considering. Such political and economic considerations no doubt have been primary factors in the emergence of a popular distinction between "pure" and "applied" science (cf. Section 2.7). This image of science has provided the basis for most attempts to relate scientific knowledge to socio-economic objectives. In this image certain 'pure' kinds of research involve the objective pursuit of truth, and should be conducted in entirely autonomous fashion, i.e. according to the regulatives of science only. Other 'applied' research is directed to the achievement of specific and practical objectives and may therefore be expected to be administered and held accountable, in more or less the same way as any other process of production or social function. The prolonged debate over the Rothschild Report in the UK and the related Australian dispute over the Philip Report are both basically disputes over where the dividing line between autonomy and accountability should be drawn and, once drawn, what form and distance of separation should be established between the two conflicting concepts.

This idea of science as being polarised into two types has in fact become very widely shared as a common sense assumption about science. As such it can be seen to encourage considerable tension between the two competing types of research. The way in which research was defined in Section 2.7 does appear to reconcile this tension somewhat through the definition of a continuum of different types of research, implicitly none of which are superior or more fundamental from a logical or sociological point of view. This is not to say that such a tension does not actually exist, however. The point is that the tension is a social product which results from the competing interests that scientists do have and the various understandings that they have internalised during professional socialisation. These competing interests may even be as the case studies indicate, nurtured within the individual scientist.

Whatever biases may be encouraged by particular organisational contexts of scientists (for example, academic as opposed to industrial scientists) the value of autonomy remains fundamental. This is illustrated by the general nature of science policy work, which despite considerable mediation by the state still preserves this fundamental value. Thus even though the establishment and subsequent growth of science policy as a distinct field of activity (with its emphasis on mechanisms for evaluating claims for research support, and the form of institution most suitable to administer research funds) reflected a concern to more efficiently direct science to desired ends,[17] the value of scientific autonomy appears to have been accepted without much question.

> "The underlying model for science policy organisation is based on the transaction concept drawn from political science; its aim is the establishment of effective institutionalised transaction processes between an independent science institution and society, via society's representatives in government. In this model there is no mechanism permitting society's interest to operate on the scientific institution, and analysis, planning and one might add, responsibility, is limited to the areas of *application* of scientific knowledge".
> [Johnston, 1976:201].

Some sociologists, however, have been aware of the nexus between science and society. Blume [1974], for example, has argued for the establishment of a political sociology of science directed towards the explanation of the contemporary politically directed, occupationally differentiated and institutionally disparate form of science, but as yet there has been little direct response to this challenge. There is of course also a well established Marxist tradition of relating science to social needs which can be traced back to Hessen and has included within its ranks such notables as J.P. Bernal, [19] and Joseph Needham, [20] but its major contemporary expression is in detailed historical studies of the way in which particular scientific theories reflect the socio-economic and cultural context. [21] Sociological case studies of the social, political and economic mediation of knowledge in the physical sciences have not, however, been particularly fruitful with the consequence that our knowledge of the process of production of scientific knowledge is still largely restricted to the fields of history and philosophy of science.

One promising response to Blume's challenge, however, has been the work of the project group 'Alternativen in der Wissenschaft' at the Max Planck Institut, Starnberg, and Weingart at Bielefeld, who have attempted to develop the concept of 'finalisation' to establish theoretically the conditions for effective direction of scientific knowledge towards research goals which are in fact, highly mediated politically. [22] This approach has been developed over several years and has resulted in at least one sophisticated and fruitful model for the formation and transformation of research objectives, and the limitations placed on the achievement of such objectives by the state of development of relevant knowledge fields. [23]

Nonetheless, 'finalisation' represents only one extreme, and rather unusual type of externally directed science, taking as its implicit model the US crash programs to land a man on the moon (the Apollo project) and to find a cure for cancer (the 'cancer moonshot'). There may in fact, be a very wide spectrum of goal orientation within science, ranging from the politically motivated crash program to a much less specific but still perhaps highly pervasive mediation of research goals by social or economic needs. Furthermore, the case for the relative theoretical closure of research which has become "externally" directed is by no means established by the small range of examples Bohme et.al. [1976] present.

Clearly, this discussion so far is not without its apparent contradictions. One of the main aims of this thesis is, however, to provide an analysis which encompasses the apparent contradiction of a science which on the one hand appears to be largely directed towards socially useful goals, and a science which on the other hand, can only function by being free of externally imposed criteria of relevance. That is, what I regard as an apparent contradiction or "paradox of relevance" in contemporary science is the difficulty one has in reconciling the fact that scientists do apparently direct their research towards socially useful goals with the attitude that science cannot proceed under conditions of negotiated criteria of social relevance. As will be shown in the case studies however, the idea that research can be considered as uniquely oriented towards this or that social goal is difficult to support in the light of evidence that research is directed towards a number of goals that differ in their orientations - some of the goals of research may be socially relevant but it may also be the case that most of the goals of *ostensively* useful research may still nonetheless be highly technical and directed away from obviously socially useful ends. Evidence suggests that some scientists may actually alternate between sub-universes that are concerned with either research or the legitimation of research - or as they will be defined in the next chapter "contexts of legitimation" and "contexts of research" which may have quite different goals and interests. This alternation between different contexts of relevance provides one possible resolution of the paradox of relevance discussed above. That is, what is relevant about research from the point of view of the legitimation of that research to others predominantly interested in the social, economic and political implications of research may differ from that which is considered relevant from the point of view of actually performing research.

3.5 Conclusions

In this chapter some of the theoretical tools of Chapter 2 were applied in the light of material drawn from mass surveys and historical analyses. The practical significance of the concepts of the goal orientation of research and the professionally inspired valuing of autonomy by scientists have been developed through an analysis of Australian mass survey data and the responses of scientists to two significant threats of "external" interference with scientists' autonomy.

In summary, it would appear that on the basis of Project SCORE data only a very small percentage of research in Australia (less than 10%) could conceivably be discussed in isolation from a goal oriented socio-economic support structure. On the basis of this kind of information, which is at least partially inspired by scientists' self images, the apparent reluctance of many scientists to come to terms with this particular social dimension of science must appear somewhat puzzling. What is it that causes the "pure academic model" of science to persist as a dominant scientific self image? One of the reasons advanced in this chapter was that science is inherently conservative, as evidenced by the apparent inertia of the Australian natural sciences. This argument relates to the general character of science as profession which requires autonomy as a central value and which encourages a highly specialised scientific training process. There is however, a more persuasive argument available that has not yet been fully developed - scientists do actually tend to bracket social considerations as "non scientific" and "external" to science. The possibility of the still overwhelmingly technical nature of most scientific research will be discussed at greater length in the case studies, but at this stage of the analysis it has been hypothesised that the strong possibility that the apparent contradiction of a science which on the one hand appears to be largely directed towards socially useful goals and yet which on the other hand can only function, so it is claimed by many scientists, in a relative freedom from externally imposed criteria of relevance may well be a consequence of the ability of scientists to alternate in their role as scientist between separate "contexts of research" and "contexts of legitimation".

So far then, we have seen that it is at least possible to examine science in terms of its directed nature, even if our knowledge about these directions is still highly generalised and possibly inaccurate. What this does presuppose however, is the existence of goals that do actually orient scientists in their research. This is a subject that we have not yet discussed in sufficient depth; that is, before one can fully understand how and why science as an institution, and a body of knowledge, is directed it is necessary to have at least some understanding in general terms of what goals are, where they come from, and just what it means to perceive a goal. Furthermore, given the complexity of the intellectual and social context of most research it seems unlikely that research will be simply oriented to single high level goals. It seems more likely that scientists operate in the context of a hierarchy of goals which may vary depending on the prevailing social context and scientists' notions of relevance appropriate to that context. The theoretical basis for this investigation of the nature of the goal orientation of scientists will be further explored in the next chapter. This chapter will develop a general proposition: all scientists are goal directed in their research and therefore science (as an institution and as a body of knowledge) can be analysed in terms of goal orientation. Only after the completion of that analysis will we be in a theoretically sound position to begin a more deeply empirical investigation.

FOOTNOTES TO CHAPTER 3

1. There is no data that is more up to date that has yet been published. The data for the period 1976-77 was not published at the time of writing and is based on partial data obtained directly from the Department of Science.

2. It must be stressed that the figures presented are heuristic, and that the Project Score surveys for 1968-69 and 1973-74 are only broadly comparable. There are several reasons for approaching these figures with some caution:

   i. The normal problems associated with sampling from a large population mean that there may be significant errors associated with some of the figures;

   ii. There were significant changes in the methods and categories used in the first two surveys.

3. The apparently large increase in basic research between the first two surveys is partially contributed by a change in categorisation by the CSIRO. CSIRO now uses a five category classification of type of activity, of which the first two categories, basic research and strategic research, together with correspondence to the Project Score basic research category. However, in 1968-69 CSIRO classified their "strategic" research as equivalent to the Score applied research category. This probably accounts for about 30% of the increase.

4. The category of the objective, "advancement of science" is defined in the 1968 Project Score Report as "a residual category of knowledge that could not be attributed to a specific objective" [Project Score, 1973, op.cit., p.12].

5. See Frascati Manual, op.cit., p.14. Note that the apparent increase in research expenditures on the "advancement of science" between 1968 and 1973 does not reflect a sudden rush of

scientists into undirected research; rather it reflects a change in how statisticians classified the data. In 1968-69 research expenditures in higher education were spread (in the classification of data) across the objectives to which the research referred while 1973-74 expenditures on higher education were all included as being directed towards the "advancement of science".

6. This statement was made directly by Mr. Fraser in 1969 during his first term of office as Minister for Education and Science in the Liberal Government. See M. Fraser, Government Approaches to Science, Canberra: Australian Academy of Science, 1969.

7. See S.C. Hill, P.J. Fensham and I.B. Howden, op.cit., pp.45-49.

8. See P.N.G. Armstrong, Stephen C. Hill and I.G. Ross, "Australian Ph.D Graduates in Science and Applied Science", Proceedings, RACI, 31 [1966], pp.149-153, and P.N.G. Armstrong, Stephen C. Hill and I.G. Ross, "Australian Ph.Ds, Specialisation Supply/Demand", Proceedings, RACI, 31 [1966], 1964 and 1972, see S.C. Hill, P.J. Fensham and I.B. Howden, op.cit.

9. There are some moves to change the character of research training but so far these have remained the work of isolated individuals, or new and as yet untried institutions, such as Griffith University.

10. S.C. Hill, P.J. Fensham and I.B. Howden, ibid, pp.53-56.

11. See, for example, R. Williams, "Some Political Aspects of the Rothschild Affair", Science Studies, 3 [1973], pp.31-46.

12. The approach taken in the Philip Report has been criticised in Hill and Jagtenberg [1977].

13. J.R. Philip et.al., Towards Diversity and Adaptability, Report of the Science Task Force to the Royal Commission on Australian Government Administration, Canberra: Australian Government Publishing Service, 1975.

14. "Ideological" in the sense of corresponding with the interests of particular social groups - that is to say, ideological more in the sense of "particular ideology" as defined by Mannheim [1972] in his essay "The Sociology of Knowledge" than in the generalised Marxian sense of relating to the interests of the bourgeoisie (- true as that may be). Like the Marxian concept, however, Mannheim's concept of ideology also implies a demonstrable distortion of reality.

15. Y. Ezrahi, "The Political Resources of American Science", Science Studies, 1 [1971], p.117.

16. Even so, I hope to show through the case studies presented in this thesis that studies of university based research need not necessarily perpetuate this image.

17. The "science policy" boom of the 1960s, directed as it was in each country by a small group of elite scientists with experience of the science-politics interface could also be interpreted as an attempt to develop a "scientific" basis for the management of science, and thus to ward off growing political determination of not only the level of funding, but the major directions of growth, of research, cf. J. Haberer, Politics and the Community of Science, van Nostrand, New York, 1969.

18. B.M. Hessen, "The Social and Economic Roots of Newton's Principia", in N.I. Bukharin et.al., Science at the Crossroads, Cass, London, 1971.

19. J.D. Bernal, The Freedom of Necessity, Routledge and Kegan Paul, London, 1949, and The Social Function of Science, MIT Press, Cambridge, Mass., 1960.

20. J. Needham, Time: The Refreshing River, Allen and Unwin, London, 1943.

21. For example, P. Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927", Historical Studies in the Physical Sciences, 3 [1971], pp.1-115, postulates the relation between the emergence of quantum physics and the hostility of strict determinism in Weimar culture; R.M. Young, "Malthus and the Evolutionists: the Common Context of Biological and Social Theory", Past and Present, 43 [1969], pp.109-145 and "Darwin's Metaphor Does Nature Select?" The Monist, 55 [1971], pp.442-503 examines

the social basis of Darwin's evolutionary theory; R.S. Cowan, Francis Galton's "Statistical Ideas: The Influence of Eugenics", Isis, 63 [1972], pp.509-528. With regard to more contemporary science, J. Slack, "Class Struggle Among the Molecules", in W. Pateman (ed.), Counter Course, Penguin, Harmondsworth, 1972, has argued that the concentration on the atypical single product, high yield chemical reaction in academic organic chemistry is a response to the needs of the chemical industry, and R. Johnston and D. Robbins, [1977], op.cit., note 13, have made a general analysis of the role of occupational values in determining the development of theory and technique.

22. G. Bohme, W. van den Daele and W. Krohn, "Alternativen in der Wissenschaft", Zeitschrift fur Soziologie, 1 [1972], pp.302-316; P. Weingart, "On a Sociological Theory of Scientific Change", in R. Whitley (ed.), Social Processes of Scientific Development, Routledge and Kegan Paul, London, 1974; G. Bohme, W. van den Daele and W. Krohn, "Finalization in Science", Social Science Information, 15 [1976], pp.307-330; W. van den Daele, W. Krohn and P. Weingart, "Political Direction of Scientific Development" in E. Mendelsohn, P. Weingart, and R.D. Whitley (eds.), The Social Production of Scientific Knowledge, Reidel, Dordrecht, 1977; for critiques see R. Johnston, "Finalization: A New Start for Science Policy", Social Science Information, 15 [1976], pp.331-336 and J.M.D. Symes, "Policy and Maturity in Science", Social Science Information, 15 [1976], pp.337-347.

23. W. van den Daele, et.al., [1977], op.cit., note 12.

CHAPTER 4: SCIENTISTS HAVE GOALS

> "This manner of clarifying history by inquiring back into the primal establishment of the goals which bind together the chain of future generations, insofar as these goals live on in sedimented forms yet can be reawakened again and again and, in their new vitality, be criticized; this manner of inquiring back into the ways in which surviving goals repeatedly bring with them ever new attempts to reach new goals, whose unsatisfactory character again and again necessitates their clarification, their improvement, their more or less radical reshaping - this I say, is nothing other than the philosopher's genuine self-reflection on what he is *truly seeking*, on what is in him as a will coming *from* the will and *as* the will of his spiritual forefathers".
>
> Edmund Husserl, The Crisis of European Sciences and Transcendental Phenomenology, p.71.

The natural sciences provide, it was postulated in Chapter 1, a paradigm case of goal directed behaviour. This was held to be of particular significance to our understanding of action in a bourgeois epoch wherein goal-rationality has absorbed value rationality. But what are the essential features of goal orientation, and what sense can we give to the idea that science is goal directed?

To begin with, one important premise of this chapter is that goals are a necessary feature of *all* action. Given that premise it makes obvious sense to reflect at this point on what our notions of science as a species of action are so far. We have considered science fairly generally thus far as a professionalised system of theoretical production. Research processes were defined as the central dynamic in this system, that is to say, our clearest definition of science as action has been given through a definition of research. If we recall our earlier definition of research as "a process of creating and transforming objects of consciousness by certain procedures in the context of thematic, interpretational and motivational structures of relevancy" (Section 2.5) it is clear that direction is implicit in the idea of relevance. In other words, research is necessarily goal oriented, as a consequence of being a process of creation within structures of relevance. Or in Habermas' terms scientific research is a species of *purposive* rational action in that it is either instrumental action, or rational choice, or their conjunction; [1] clearly, purposive action means in this sense action which to some extent devised in advance.

4.1 The "Common-Sense" notion of goals in scientific research

Given the fact that modern life is so dominated by an instrumental type of behaviour which is concerned with means rather than ends, it is hardly surprising that social scientists have generally tended to take the nature of goals for granted. That is, although social scientists may discuss, at times, the processes by which particular goals are formed (for example, in political and managerial analyses) the level of theory which supports these analyses is usually low. [2] In science studies the relatively "low level" goal of the solution of particular scientific problems has tended to be the primary focus of investigation, and other levels or kinds of goals which may shape the research process have been neglected. Moreover, the existence of "problems" has often been taken for granted by physical scientists and social scientists. This tendency no doubt springs from the neo-positivistic assumptions of most practicing scientists who tend to regard scientific problems as arising spontaneously from the interaction of individual scientists with physical nature. This sociologically naive view which is deeply entrenched in the standard scientific epistemology that is communicated to young scientists during their socialisation has remained largely unexamined by the sociology of science. It is only with the development of the post-Kuhnian sociology of scientific knowledge that the perception of a scientific problem has been seen as a phenomenon in need of sociological explanation.

The aspect of this naivety most central to this thesis is the uncritical acceptance of scientists' goals as unproblematic aspects of individual and collective consciousness. As a consequence, neither goals nor the relationship between means and goals (or "ends") has been the subject of serious investigation in the science studies literature. The dominant approach has been a functionalist analysis of the evolutionary movement of means towards "immaculately conceived" goals. It is ironic however, that amongst sociologists of science the decline of interest in the more traditional Mertonian functionalist analysis of science as an institution has apparently entailed a decreasing awareness of goals as part of the institutional context of scientists.

In general references made to goals in studies of science and scientists are sparse, or made, in abstract terms, as part of a more general analysis. Thus, Merton [1973:chapter 13] sees the goal of science as "the rational pursuit of truth". For Sklair [1973:66], "the charter or purpose of science" is of three types viz., the quest for knowledge for its own sake, for alleviating human suffering and satisfying the needs of mankind, or to provide an economically rewarding career. Richter suggests:

> "The goal of science, as commonly recognised today, involves the acquisition of systematic, generalised knowledge concerning the natural world; knowledge which helps man to understand nature, to predict natural events and to control natural forces" [1972:14].

Ravetz [1971] has made a considerable effort to clarify the notion of goal by developing a hierarchy of "final causes" that determines the goals of the research task. He distinguishes between goal, function and purpose:

> "The task itself has a goal, which is conditioned more or less strictly by the function which will be performed by the result of the accomplished task; and this in turn is governed by the

> ultimate human purposes which are expected to be served by the performance of that function".

The goal is the solution of the research problem.

The most detailed analysis of goal direction in science is offered by the sociologist, Hagstrom [1965] but in general the concept is used in a common-sense way to explain structural change, the role of fashion and the processes of disciplinary differentiation and social control. Thus, although Hagstrom observes that:

> "Segmentation begins with cultural change, the appearance of new goals in the scientific community. Of course, new goals do not spontaneously appear: scientists actively seek them" [ibid:222],

he provides few indications of the origin of the goals and continues by examining the way scientists respond to cultural change once it has occurred. The uncritical use of the concept is highlighted by the conflation of "goals" with "problems". Thus, the above quotation continues,

> "Those who discover important problems upon which few others are engaged are less likely to be anticipated and more likely to be rewarded with recognition" [ibid:222].

Where do these goals come from, and how are they formed? Hagstrom, adhering to an internal/external demarcation, distinguishes between goals arising inside and outside science:

> "When the relative importance of goals is easily ascertained by generally accepted criteria, or when the goals are given by non-scientists, there will be little play of fashion. In many of the applied sciences, where the goals arise outside of science and the criteria of success are usually given by non-scientists, scientific fashion is perhaps least important. In the empirical sciences, especially those with a more or less rigorous theoretical framework, the goals arise within science, but in many respects they appear to be "given" in the confrontation of theories by "nature" [ibid:180].

The change of goals directed from within science is due primarily to the action of leaders:

> "The orderly succession of goals in a discipline is the sum of individual responses to a situation being changed by discoveries. Changes in the goals of individuals are facilitated by the tendency of scientists both to seek social validation of their goals and to follow

> the lead of outstanding men . . . The ease with which physicists can change the goals of their discipline is linked with the structure of leadership in the discipline. While the ease of determining the really important problems makes it easier to spot leaders, the existence of leaders facilitates the orderly succession of goals" [ibid:186].

Hagstrom makes other distinctions between types of goals, but not in any systematic fashion. Thus goals may be "short term", i.e. specific problems being researched [ibid:176], "traditional" disciplinary goals, as for example the purely biological goal of understanding life as a function of the cell [ibid:193-4], "applied goals" such as the pursuits of industrial and government laboratories [ibid:220], and motivational goals such as incentives, particularly recognition [ibid:227]. Implicitly, all the objects of competition between individuals and organisations are treated as goals. For example, position, promotion, research facilities and graduate students are scarce resources to be competed for, i.e. goals to be achieved [ibid:163].

The recognition of different levels and types of goals represents a considerable advance, but by failing to distinguish between goals and problems, and even more by linking this analysis with a Mertonian typology of five ideal types of scientific performer Hagstrom is committed to a static and normative analysis. That is, Hagstrom's analysis of goals is hindered by the adoption of a functionalist stance, whereby goals and changes in them can be interpreted only in terms of maintenance of particular patterns of meaning and action. This would appear to presuppose an essentially conservative definition of goal.

The idea of different levels and types of goals is still a rather abstract notion, however. Two further steps are required before we can explore the goal direction of scientists in a directly empirical fashion. First we need to define the institutional context of goal orientation in the physical sciences - this forms the subject of the next section. On that basis we can then proceed to explore the actual nature of a goal per se. This latter subject has been delayed in order that a more detailed analysis of the mechanisms of consciousness can proceed in adequately sociological context.

4.2 The institutional context of goal direction in the physical sciences

Although our discussion of goals has, so far, been in general terms it is important to stress that this is not meant to imply that "science" is profitably characterised as having universal goals. Thus, for example, although one can speak of a goal such as "the advancement of knowledge", just what will be counted as knowledge may vary from discipline to discipline. Therefore, along with Richter [1972:15] I am rejecting the notion that any single goal can be fruitfully applied to the whole enterprise of science. As Whitley [1976] and Pantin [1968] have emphasised different sciences have developed distinctive social and cognitive structures. Thus different sub-universes of science will have different goals and different traditions; nonetheless an investigation of these goals can provide useful insights into the general nature of science both cognitively and socially.

Implicit so far has been the idea that scientists actually work within the context of a hierarchy of goals that will develop through time. In this view it is obvious that there can only be a relative distinction between means and ends - one goal or end can be considered as a means to another goal, despite the existence at any one time of a structured hierarchy of goals. It seems difficult, therefore, to fully distinguish, along with Ravetz, a goal from a purpose (cf. Section 4.1). What one can do however, is distinguish between different levels of goal (including "purposes"). Thus, a scientist may have an over-arching goal which gives some relatively "ultimate" meaning to his research, together with a series of "lower" level problem centred goals.

Some progress has in fact been made in explaining how cognitive and social structures, in part determined by the elite members of a research community, shape the range of appropriate research tasks open to the "autonomous" academic scientist. [3] The way in which the apprentice, from student to post-doctoral fellow, is presented with a research "package" which closely defines an appropriate set of research problems has been usefully explored by Whitley and others. [4] Less attention has been applied to the work of scientists in industrial and government environments, though here it is clear that the research tasks are equally prescribed, though in a more overtly hierarchical or bureaucratic manner and with a more immediate orientation to the objectives of the organisation. While studies of the research task are of undoubted value, and will be empirically pursued in this thesis, I am also concerned with an analysis of the extent to which high level goals form a part of the structure determining research tasks (even if, as the case studies will suggest, the level of effect may be slight).

In the last section we concluded, on the basis of Hagstrom's rather unsystematic evidence that particular goals may vary greatly in type, level of application and origin. The work of Thomas Kuhn provides us with suggestions of a much more systematic nature. One of the major implications of Kuhn's model of a paradigm was that cognitive structures operate at different levels and given that sociologists and philosophers of science appear to have accepted this without question it is reasonable to infer that goals, which to a large extent will express themselves through the cognitive structures, may also operate at different levels. As we mentioned in Section 2.7-2, goals at the highest level form part of the "metaphysical" component of scientific knowledge - "the overall system of values and beliefs which serves to justify and integrate the scientific activity with other systems of production . . . and provides a general world view" [Whitley, 1975:41]. Such high-level goals need not be consciously associated with all phases of scientific work; they may be sedimented quite deeply in tacit background knowledge internalised through socialisation processes. Nevertheless they may provide some influence on scientific research. For example, Mullins [1972:55] identified a scientifically relevant high level goal of the phage group as determining "the secret of life".

Goals may also be formulated directly at a lower sub-disciplinary level of cognitive structure - for example, at the level of specialty concerns described as: "the general problems or purposes of conducting the activity seen in terms of a particular definition of reality which may incorporate a number of evaluative frameworks". [5] At this level goals may take a rather more concrete, and explicit form as, continuing the phage group example, the determination of "the mechanisms by which genetic information is transferred". [6] Other examples include plasma physics, where the goal is one of understanding the properties of plasma sufficiently to allow continued controlled fusion with a positive energy balance or biotechnology, where the goal is the artificial mutation of microorganisms suitable for the manufacture of industrial products. [7] Cognitive structures at lower levels, namely explanatory models, techniques and research practices, may contain expressions of the higher level goals but the latter are unlikely to operate directly at this level.

Goals may also be of rather different types. Thus some goals may take the form of very highly theoretically mediated expressions of socio-economic and cultural context such as operate in physics and biology. [8] At the other extreme goals may be much more direct expressions of non-scientific interest groups such as sectors of government, industry, or the public, as seen in the development and direction of fields such as computer science, geology, tribology, toxicology and environmental science.

On the basis of our discussion so far three general points emerge as a broad summary of the institutional context of goal direction in the physical sciences: scientists are directed in their research by goals which are -

1. Established as the result of social and political processes which involve dynamic interaction between interest groups which may involve or exclude direct scientific interests, and which may be directly or indirectly perceived by scientists.
2. Mediated by scientific, social, economic and political considerations and expressed at varying levels of generality; these mediated versions may be expressed within "official" statements of research programs or they may deeply be embedded in the structures of relevance of research.
3. Dynamically linked to an evolving body or bodies of scientific knowledge in such a way that research and the goals of research are only analytically separable; both cognitive and social aspects of research are directed and constrained by orientation to goals which are posited and potentially continually redefinable in terms of changing theory, techniques and conditions.

In the concluding sections of this chapter the constitution of this institutional context will be discussed more deeply in terms of reference group theory and the segmentation of the sub-universe of the research program. At present though, now that we have some broad guidelines about the institutional context of goal direction, it is time to deal with goals at a more deeply theoretical level. In the following sections I will attempt to re-open an appreciation of goals that penetrates beyond our pragmatically useful taken for granted assumptions about the nature of goals. Aspects of the philosophy of science and phenomenology are useful at this point in that they provide an initial point of departure.

4.3 Back to basics: some directions from the philosophy of science and phenomenology

As discussed earlier, one of the distinguishing features of the literature about science is a common sense approach to the subject of goals. Dissatisfaction with this situation provides sufficient reason for casting further afield for additional insight. In this search for clarification I propose to "go back" to a more phenomenologically inspired consideration of consciousness, action, and what it means to perceive a goal. This move to fundamental issues has also been stimulated by recent moves within the philosophy of science where it is possible to see the birth of a realisation that it is important to become concerned with how scientists actually do proceed rather than only with how they ought to proceed. Hanson, Polanyi, Kuhn and Toulmin are the most outstanding proponents of what has become known as a "new philosophy of science" [Shapere, 1966]. The individual contributions of these figures are less important than their "revolutionary" potential as seen through a phenomenological eye. [9] As Kisiel [1973:267 and 269] puts it,

> "These basic theses of the new philosophy of science - its emphasis on historicity and discovery, on the historical situation of a

> finite context of presuppositions within which scientists do their work, a situation which not only limits but also provides a scope of possibilities for discovery - all of these themes strike resonant chords with the phenomenological tradition.
>
> Man's understanding of Being is an ontological process before it is a mental process, in as much as it is more of Being than of man. The discussion no longer gravitates toward man, but toward . . . the locus of humanity in which man dwells.
>
> What the epistemology of the new philosophy of science variously calls presuppositions, paradigms and conceptual frameworks now becomes the world, the meaningful context in which man lives, moves, and is".

Basically, these "new directions" involve (amongst other things) the setting up of an old problem in the new context of the natural sciences: how is it possible for mind, body, society and nature to come together in any process (perception, knowledge, work, whatever). As the biologist and philosopher Gregory Bateson [1973:285] puts it,

> "It is awkward to refer constantly to both epistemology and ontology and incorrect to suggest that they are separable in human natural history. There seems to be no

> convenient word to cover the combination of these two concepts. The nearest approximations are "cognitive structure" or "character structure", but these terms fail to suggest that what is important is a body of habitual assumptions or premises implicit in the relationship between man and environment, and that these premises may be true or false".

Kisiel attempts to take the theme of this "new philosophy of science" to an investigation of hermeneutics and a language based understanding of science, but I feel that essential as such discussions are, it is important to get back to the problem of what it means to perceive a goal. This search for meaning will be pursued as a primarily sociological problem, however. In this perspective the relationship of individual perception of meaning to institutionalised structures of meaning is a fundamental concern (as expressed in the quote above from Bateson). Our investigation will be based nonetheless on the assumption that sociological analysis must treat individual consciousness as a phenomenon of primary importance. For this reason I have chosen phenomenology as a theoretical starting point for our analysis of goal perception.

Phenomenology starts its treatment of perception and the problem of the "separation" of the knower and the known with the concept of intentionality. It is because of the intentionality of consciousness that we are in direct contact with the physical and social world. And in our present context the intentionality of consciousness makes the perception of goals possible.

4.4 The intentionality of consciousness

One of the basic postulates of phenomenology is that consciousness is intentional, that is, all our cogitations are essentially and necessarily cogitations of something; they refer to objects in consciousness [Schutz, 1973:106].[10] Here, "object" must be understood in the very broadest sense as to all those things encountered by consciousness. These "things" may range through material, cultural and social objects and include scientific constructs (such as matter, energy, force, atom, propositions, models, systems, etc.) and ideal entities of every kind and description [cf. Gurwitch, 1974:213].

In other words, the "object-as-intended" is a concept that is basic to consciousness. It is important to note that intentionality is not merely a directing of attention, for directedness in this sense merely denotes a phenomenal feature of consciousness, that is, directedness is merely descriptive. Rather more than this is implied. The various directed consciousness acts somehow cohere, and we are aware of objects which persevere over time, which have properties and which mean things to us. This is where Husserl's concept of noema provides a solution to the old problem of explaining how our sense perceptions hang together. The noema is a unifying concept which deals with the way consciousness has structures which organise perception into meaningful structures; noema is the object "as it presents itself", or "as it is intended" or "meant" [Gurwitch, 1974: 229].

But, it might be objected, our perceptions change from time to time too, even though they are "meaningful" perceptions. So how does the concept of noema account for any feelings of continuity of meanings that I may have? In this respect Husserl further distinguishes between individual noema and a "noematic nucleus": any two noemata, their differences notwithstanding may have a certain stratum in common which Husserl denotes as a "noematic nucleus" [Gurwitch, 1974: 232]. Thus, a house may be torn down but even after its destruction it may be remembered as presenting itself under various aspects over time. These memories are related insofar as they relate to the changing fortunes of a house; there is, so to speak, a theme to our perceptions. This idea of a theme applies equally well to our dealings with future events. If we make plans and project goals for our actions some of these goals may change with events, nonetheless our plans, or projects of actions, may still be related by virtue of retaining a similar "over-all" direction, or orientation. This subject will be pursued further after "goal" has been more fully defined.

This still leaves us to define consciousness: according to Gurwitch [1974:233] consciousness is a correlation between temporal psychological events and atemporal, ideal entities which are categorised as "objects-as-intended", or in Husserl's terms a correlation between noesis and noema. That is, the noetic and noematic aspects of the intentional relation determine each other and each can be only understood in the light of the other. This becomes more obvious if we consider the noetic as subject-in-relation-to-the-object and the noematic as the object-in-relation-to-the-subject. Clearly there is no object unless it is object for some subject and no subject unless it has a world as its object [cf. Schmitt, 1967].

It is one thing though to observe that consciousness operates in terms of meaningful relationships but just how this organisation of consciousness comes about is still not perfectly clear. Rather than just taking this organisation for granted, or dismissing it as a psychological problem, I propose to briefly introduce a few of Michael Polanyi's ideas by way of further accounting for this process of organisation. This will be important not only for the beginnings of an understanding of what it means to perceive a goal and where goals come from, but it is also important for the beginnings of an understanding of how the production of any kind of knowledge comes about.

In The Tacit Dimension, Polanyi gives a good statement of his position. "We know more than we can tell", he says. Reflecting against work in the field of gestalt psychology which demonstrated that we may perceive an object by integrating our awareness of its particulars without being able to identify these particulars, Polanyi recasts Gestalt notions:

> "Gestalt psychology has assumed that perceptions of a physiognomy takes place through the spontaneous equilibration of its particulars impressed on the retina or on the brain. However, I am looking at Gestalt, on the contrary, as the outcome of an active shaping of experience performed

> in the pursuit of knowledge. This shaping or integrating I hold to be the great and indispensable tacit power by which all knowledge is discovered and, once discovered, is held to be true" [1967:6].

This process of integration which makes knowledge possible is very similar to a phenomenological treatment. Polanyi introduces slightly different terms, however: in cognition we attend *from* particular perceptions *to* some object which makes sense of (or gives meaning to) these perceptions. More generally, in acts of tacit knowledge we attend *from* something *to* something else (and this, Polanyi rather dubiously implies, is to be distinguished from the mechanical applications of a rule or technique in "non-tacit" knowing). The parallels with the phenomenological tradition are striking:

> "Phenomenological analysis shows, however, that there is a pre-predicative stratum of our experience, within which the intentional objects and their qualities are not at all well circumscribed; that we do not have original experiences of isolated things and qualities, but that there is rather a field of our experience within which certain elements are selected by our mental activities as standing out against the background of their spatial and temporal surroundings; that within the

> through and through connectedness of our stream of consciousness all these selected elements keep their halos, their fringes, their horizons" [Schutz, 1973:112].

Perhaps Polanyi might have benefited from a reading of Husserl and Schutz.

Polanyi doesn't go much further in his analysis of consciousness. He is not particularly interested in the social aspects of the longer term directions in the production of knowledge and therefore any discussion of the goals of scientists as important elements in the field of cognitive possibilities would be a theoretical imposition on Polanyi's work. Furthermore, the idea of tacit knowledge actually enables him to mystify the processes of knowledge production. Quite reasonably he argues throughout his works that any theory (or knowledge) can only be constructed by relying on prior tacit knowing and that therefore all knowledge has a non explicit, tacit component. But why stop there? Can we know more about this "tacit dimension"? Can we, for example, identify some kind of structure in the organisation of consciousness, can we identify different levels of structure, are there social factors involved, and so forth. [11] Polanyi uses his analysis as the basis of his belief in the necessary autonomy of scientists (for apparently the mysterious processes at work in scientific creativity need protection from disturbing influences, such as left wing political interference. [12] And of course it follows that any efforts to plan science will be somewhat futile in the face of this precondition of autonomy. It is tempting to speculate that it might have been very convenient to suspend analysis of the tacit dimension at a point where the possibility of the existence of social factors as important to the content and organisation of consciousness might seem remote. In this way the "cognitive purity" of science remains unthreatened and scientific knowledge (and its producers) can continue to remain as a paradigm (in the sense of being an example to all).

Nonetheless, Polanyi has made an important contribution to the analysis of theoretical knowledge production in the physical sciences, in that at least the possibility that there may be factors at play in the organisation of consciousness that are not necessarily explicit in the objectivations of consciousness. It remains for others to extend the analysis of these factors beyond the level of individual "taste" or other aesthetically biased criteria. Also, his use of the "from-to" terminology is heuristically useful as a reminder that knowledge is produced on the basis of a movement in consciousness *from* a field of possibilities *to* some intentional object.

In conclusion, there seems to be at least one good reason why the treatment of consciousness that has been presented so far seems to fall short of explaining how the production of theoretical knowledge occurs. And this is, quite simply, that the development and organisation of consciousness has still not been adequately treated in its social aspects. It is possible to approach this problem from a variety of directions - for example, Durkheim, Levi-Strauss or Mead - but from the standpoint of phenomenology Schutz is a most promising starting point. Schutz and Luckmann's The Structures of the Life World is of course, devoted to the problem. Unfortunately though, the approach in their book is more focussed towards the relationship between individual and society and the nature of the natural attitude than to the problem of social action and the production of theoretical knowledge (although there are many insights present in the book as the following pages will testify). The social nature of the mode of the intentionality of goals in scientific research forms the subject of the remainder of this chapter.

4.5 Scientific research as projects of action

One of the inspiring things about Schutz's [1973] essay "Choosing Among Projects of Action" is that the basic ideas lend themselves to a description of scientific research. It is in fact quite fruitful to consider scientific research as a type of action for with this focus it seems at least possible to overcome the normative idealism of traditional descriptions of science.

The term "action" designates human conduct as an ongoing process which is devised by the actor in advance (but subject to constraint). Those aspects of the process which are preconceived and posited as goals or ends allow us to speak of a "project" of action. The term "act" is used by Schutz to designate the outcome of this ongoing process, that is, the accomplished action. Schutz [1973:67] is careful to point out that not all conduct is purposive. "In order to transform the forethought into an aim and the project into a purpose, the intention to carry out the project, to bring about the projected state of affairs, must supervene". This enables us to distinguish mere "fancying" or fantasy from projections which are made with the intention of "gearing into the outer world" as, for example, would be required in scientific research.

In the case of natural scientific research in advanced industrial societies, the concept of project needs to be encompassed by a broader concept, the research program (see Section 2.2). This is considered necessary in order to take account of the way that scientific research is becoming increasingly capital and labour intensive, and that consequently such research is tending to be less the activity of isolated individuals working on discrete projects and more a co-ordinated activity involving groups of individuals sharing resources and working towards shared goals, or at least working within some kind of loosely co-ordinated framework. That is, research in the natural sciences is highly institutionalised compared with other types of research and action, in general. Although it does not necessarily follow that shared resources entail co-operation, it should be clear that the horizons of meaning possible in the natural sciences are to some extent the product of the shared tools (theoretical and material) employed in research. The term "horizon of meaning" is taken here as broadly encompassing perceived theoretical and practical possibilities, the kinds of problems considered as relevant in particular disciplinary orientations, the theories, techniques and instruments used and referred to, and the general attitudes, values, beliefs, etc., involved in particular programs of research.[13]

In conclusion, the research program is defined here as the institutionalised form of thematically, motivationally and interpretationally related projects of action.

4.6 The concept of goal

So far we have seen how the consciousness and action of individual scientists are socially determined by virtue of arising in the context of shared patterns of meaning and action. Before we can go on to discuss scientific research from a more empirically based perspective however, we need to add to our understanding of the goal directed nature of scientific action. This entails, in the first instance, a more general understanding of the nature of goals as components of all action. On that basis we will then be in a position to expand our analysis of the institutional context of goal direction in the physical sciences (through discussion of reference groups and legitimation). These moves are the final steps before the beginning of an empirical exploration of the institutionalisation of scientific research.

This concept of project incorporates a concept of goal since a defining feature of action undertaken in programs of research is that such action is to some extent devised in advance. Such action has a pragmatic motivation of "in-order-to". According to Schutz and Luckmann [1974:213] the goal of an act "motivates the projection of the act in its various phases, including the beginning: that is, the goal of the act precedes the actual action. The act ensues *in order to* reach the goal" (my italics). In other words, as mentioned earlier, any analysis of action has to deal with goals since goal-orientation is a defining quality of all action; certainly it appears possible to construe any motivated behaviour as being "in-order-to" do something (even if this may involve confusing the "in-order-to" motive with the "because" motive [Schutz, 1973:69-72; Schutz and Luckmann, 1974:208-215]. And, of course, even an assertion that aimless behaviour is *not* goal directed requires an understanding of that which it is not.

It was also mentioned earlier that sociologists have tended to take a common-sense approach to the subject of goals - that is just what a goal is, how it is possible in consciousness, and where it comes from has been given scant attention by sociologists. This neglect is not even absent from the phenomenological tradition despite its self professed preoccupation with the structures of consciousness. Thus, although the treatment of action presented by Schutz and Schutz and Luckmann, for example, depends on the concept of a goal as outlined above, it is quite apparent, I believe, that we still do not have an adequate theoretical analysis of what a goal is. In this section I shall attempt to provide a more satisfactory treatment. I have attempted here to show how phenomenology can be given something of a dialectical perspective in order that the potentially dynamic nature of goals is brought to attention.

In the definition given above the relationship between motive, goal and act is still obscure: "the act ensues in order to reach the goal" presupposes the projection of a goal, which is still an unknown quantity. What is missing is a sense of "objectification", that moment in consciousness where the acting subject establishes distance from his/her producing and its products. Or as Berger and Pullberg

[1965:200] put it,

> "By objectification we mean the moment in the process of objectivation in which man establishes distance from his producing and its product, such that he can take cognizance of it and make it an object of his consciousness".
>
> "By objectivation we mean that process whereby human subjectivity embodies itself in products that are available to oneself and one's fellow men as elements of a common world" [ibid:199].

Goals then, are the objectifications of the in-order-to motive. Action can only occur if the subject can establish distance from his producing and its products through the awareness that the nature of one's future world can be influenced by the individual acting subject. Social action and the gratification of particular needs and desires is only possible through this process of distancing between the subject and his needs and desires through a process of giving form to possibilities, to potential which has not yet been realised. These forms are the goals of action, which are objectifications in a flux of "becoming" which constitutes the life world.

Objectivation and objectification are fundamental properties of human consciousness - clearly, human existence cannot be conceived without them. As Berger and Pullberg stress, however, objectivation and objectification do not fully define the human condition - they are *a priori* necessary but they do not account for a world which is *de facto* characterised by a broken unity between the act of producing and its products which constitutes a world of "things" which appear alien and malevolently powerful. That is the bourgeois epoch is characterised by alienation and reification; according to Berger and Pullberg, "reification is objectification in an alienated mode" [ibid: 200]. This has obvious consequences for our understanding of the nature of goals: we can expect goals to be reified in the "normal" processes of social action. That is goals will assume the characteristics of "things" separated from the means used in the processes of their realisation and lose their quality of freely evolving objectifications of creative human potential. [14]

This alienation is reflected in the relationship between the individual and institutional levels of social life. A necessary consequence of socialisation is that any individual internalises various patterns of meaning and action as definitive in various ways of the meaning and possibilities of individual and social life. To the extent that an individual is prevented or unable to consciously share in the interpretation, re-interpretation, and change of these patterns s/he can be described as "alienated" in the Marxian sense of the term. That is, an alienated individual has no sense of creatively appropriating his/her environment for the production and reproduction of daily life. This means that the various institutional forms that provide the context of daily life become empediments to the individual. The various stocks of knowledge and socially sanctioned notions of relevance that provide this context stand between the individual and his desired projects. Individual life is stifled by institutionalised rules, values and knowledge, which ironically continue to provide a context which is internalised as the naturally occurring human condition. The question of how to break this vicious circle is beyond the scope of this thesis, but I will go on to discuss aspects of the manifestation of this alienated relationship between the individual and institutional level of social life. From the descriptive point of view that will be adopted these manifestations of "alienation" will however, become part of the phenomenology of the scientific life world. That is to say, since it is taken for granted that alienation is manifest in all aspects of social life, it is not necessary in this thesis to continue to use the term in describing the institutional context of the scientific life world.

Goals have been defined here as objectifications of the in-order-to motive, albeit that this objectification occurs in an alienated mode. However, little sense has yet been given to the relationship between "motive" and "action" apart from the general assertion that action is motivated. As we have seen, Schutz conceives motivation as having two aspects: a "because" motive and an "in-order-to" motive. The in-order-to motive is defined in terms of the attempted *realisation* of needs, disires, etc. whereas the because motive is defined in terms of "habitual knowledge" which is brought to bear in any situation. This habitual knowledge forms a "syndrome" or "attitude" which consists of "expectations, hypothetical relevances, plans for acts, skills, . . . and frames of mind" [Schutz and Luckmann, op.cit:217].

Thus, in an example developed by Schutz and Luckmann, a man may hit a coil of rope with a stick in order to see whether it will move and because he is afraid of snakes. The two types of motive, whilst being related in the totality of any situation at any moment are nonetheless quite distinct in consciousness:

> "Motive may have a subjective and objective meaning. Subjectively it refers to the experience of the actor who lives in his ongoing process of activity. To him, motive means what he has actually in view as bestowing meaning upon his ongoing action, and this is always the in-order-to motive, the intention of bringing about a projected state of affairs, of attaining a preconceived goal. As long as the actor lives in his ongoing action, he does not have in view its because motives. Only when the action has been accomplished, when in the suggested terminology it has become an act, he may turn back to his past action as an observer of himself and investigate by what circumstances he has been determined to do what he did" [1974:70].

In this process of objectification goals are *intended* as particular projections of actions, but at the same time goals are not essentially different to other objects in consciousness.

As discussed,

> "Action of any sort that involves gearing into an external world cannot proceed without this facility of consciousness to objectify parts of the world as being "other" to self, that is as capable of being reflected against in consciousness. It is just this facility of consciousness which enables us to relate to the future as part of a field of possibilities within reach, however distant" [cf. Schutz and Luckmann, 1974:36-41].

As intended objects goals are present with horizons of meaning, but these horizons are significantly more involved with location in time than with the horizons of meaning surrounding the fairly static and unreflective perception of objects that tends to predominate consciousness in the natural attitude. For example, it is important to know how soon a goal is likely to be realised in order to adjust other projects of action so that difficult situations are avoided; thus the expression "first things first". In this respect, the further a goal is located away from the present, the more empty its horizons of meaning tend to be. These horizons are "filled in" by the knowledge and experiences generated as efforts are made to realise particular goals. Thus there is a sense in which goals involve a "diminished reality" insofar as the largely empty horizons surrounding a goal located at a considerable distance in time give a feeling of an embracing openness, perhaps emptiness. And then there is a feeling of "realisation", as if the goal becomes more real, more concrete, as one "works towards" a goal by filling in its surrounding horizons with the products of labour.

So far this treatment of the intending of goals has been somewhat voluntaristic and linear, as if an individual were suddenly seized with a burst of energy which led to the projection of a goal and the subsequent engaging in some kind of action. This impression has largely resulted from the attention we have given to individual consciousness as a starting point for a more in-depth analysis. We have however, raised the subject of forces that are perceived as external to the individual through our earlier discussion of alienation and reification. At that stage we were in effect presupposing the effects of a bourgeois epoch. Let us, then, look more closely at the institutional context of goal orientation.

In this respect the salient features of scientific action are that goals may be institutionally imposed on scientists as well as being "spontaneously" produced in day-to-day activities. In this situation of "imposition" it should be stressed that the internalisation of goals which are encountered by individuals as existing "out-there" as objectifications of the "in-order-to" motives of the others in the individual's environment is part of the process of orientation towards goals such that collective activity can occur. This process of internalisation is at least in principle continuous and consequently an individual's understanding of a goal is always in a process of "becoming" in the context of the changing cognitive and social structures that provide the context and "ground" for action. The meaning of a particular goal will very much depend on the particular, concrete, activities that the individual is engaged upon. It seems possible to generalise and say that goals are in a sense simply the leading edge of a dialectic between theory and practice that constitutes action in any universe of meaning. In all cases, however, the concept of the in-order-to, or pragmatic, motive only makes sense in a world of subjective meanings, albeit that this is only possible by virtue to a co-existent world of objective (that is, shared) meanings.

The postulation of horizons of meaning surrounding a goal is taken as implying that a goal expresses aspects of the processes and cultural resources involved in its genesis - the particular content of a goal expresses particular interests, ideologies, attitudes, values, beliefs, etc. That is to say, goals necessarily express the institutionally and individually defined structures of relevance that define the context of their genesis, or in other words, as products of given, historically located fields of experience, goals reflect the "now" of their origins. Furthermore, the objectification of a particular goal (by either an actor or an observer of an actor) necessarily involves stocks of knowledge that are at hand - indeed, the goals of scientific research often result in the "mere" filling in of details in existing stocks of knowledge. [15] In general, projecting always refers to the actor's [16] stocks of knowledge at the time of projecting and carries along particular "empty" horizons of anticipation, subject to the assumption that the projected act will tend to go on in a typically similar way to related past acts known at the time of projecting [cf. Schutz, 1973:72]. These "empty" horizons do not however escape institutional constraints. The individual perception of uncertainty or ignorance is only the consequence of an awareness of the space "between", as it were, existing normative patterns. These spaces may be, to borrow Kuhn's sense of "paradigm", part of an *overall* pattern which just happens to come with pre-defined "holes" (or "puzzles") which are capable of being filled (or "solved") by extension of the paradigm. In much rarer cases, the spaces may be perceived as completely "anomolous" in which case the existing conventions will not suffice. In Kuhn's terms, this event may lead to a "revolution" or "gestalt" switch; in the terms of this thesis the switch is to a different sub-universe of meaning. The conditions for the establishment of a "new" sub-universe are however, beyond the terms of this thesis.

Despite the impression that may have been given so far, goals are not necessarily perceived as objects within the tension of a wide awake consciousness; in other words, individual actors may not be fully aware of the goals which orient their behaviour. For example, it seems quite reasonable for a sociologist, say, to claim that one of the goals of a scientist is to protect particular areas of knowledge from "outside" interference, even though the individual scientist may not be aware of this orientation in his action. That is, some goals are deeply embedded in institutionalised actions, and are taken for granted as the way things are typically done. In general, the concept of goal is not being tied to perception in a particular consciousness as defining its "reality".

As suggested, some goals may be deeply embedded in long established patterns of meaning and action. Nevertheless, although it seems possible that the meanings of particular goals may remain relatively invarient over time, it is difficult not to generalise and define goals as necessarily caught in a flux of meaning shifts. As stocks of knowledge, interests, values, etc. change, so too will the way that goals are intended change. Goals are thus created and recreated through time and encountered and re-encountered as slightly, or massively different objects. Goals and themes of action may change, undergo relative "displacement", or be forgotten over time. In summary, we may speak of the formation and change of goals as being part of the dynamic processes of the evolution of projects of action.

The somewhat tension free tendencies of phenomenologically inspired thinking have already been commented upon, but at this stage in the beginnings of our appreciation of how any individual's cognitive field is structured by virtue of the social forces which affect action, it will be instructive to review the concept of intentionality. In this process of finding and making meaning there seems to be an implication, in the writings of Schutz, Schutz and Luckmann and most phenomenological treatments of consciousness, that consciousness in any given province of meaning is a relatively continuous process, as if meanings were steadily projected on the screen of consciousness as one rolled through life. On the contrary though, it has been suggested that the meanings that are comprehended as making sense of action are often fragmented and inconsistent (and often perceived as such). Even aside from this, parts of the horizons of meaning may be more densely populated than others, not simply because of familiarity with a particular situation and lack of familiarity with another, but because the process of projection of action involves a "narrowing" of view with the directed translation of what appears relevant at the present into an enlarged horizon of future possibilities, only some of which may be realised. That is the (in principle, at most) broader horizons of the future are never perceived as completely homogenous but are, in the case of a life world subject to any level of institutionalisation, divided between goals and their associated structures of relevance. [17] To put it more picturesquely, these divisions correspond to the relative condensations of meanings in one's horizons which enable one to take interest in particular themes of action. [18]

But how do these condensations of meaning come about? In the case of the natural scientist, in particular, is it possible to speak more explicitly about the processes which give rise to non-uniform and perhaps even fragmented horizons of meaning? On the basis of the material in Structures of the Life World not much more can be said; The Social Construction of Reality does contain some useful generalisations about the process of institutionalisation, but nothing which can be immediately placed in an empirical context.

What is required at this point is a further exploration of the institutionalisation of individual consciousness and action and the development of concepts that refer to the relationship between individual consciousness and processes of institutionalisation. Symbolic interactionism shows considerable promise in this respect. In particular, the concept of an orientational reference group does provide this required dynamic aspect to our discussion of institutionalisation. In the next section Manford Kuhn's notion of orientational reference group will be discussed as potentially relevant to our understanding of the social forces involved in the evolution of a structured cognitive field.

4.7 The professional orientational reference group

So far the concept of professionalism has been developed as a way of accounting, in a generalised way, for social control in science. Whilst Johnson's concept of professionalism and in particular "collegiate control" are useful at a general level, Johnson's work does not provide an adequate theoretical basis for understanding the reproduction of professionalism through professional socialisation or for understanding the social-psychological dimension of what Berger and Luckmann term "universe maintenance". Towards these ends, the concept of professional orientational reference group will be developed in this chapter as a concept that is meaningful at a level of symbolic interaction.

In Section 2.7-2 the cognitive field of a scientist "in the context of research" was described as dominated by an interpenetrating hierarchy of levels of structure which range through a diffuse level of metaphysics, a theoretical level, a level of subject concern and a technical level of procedures and techniques used during research. These structures are in fact, continually mediated and oriented by various reference groups - in this way the meaning of the various sub-universes within which scientists operate (disciplines, specialties, research programs) are defined in a process of interaction with other professionals and relatively anonymous but symbolically effective reference groups.

Professionalism operates as a type of social control through the agency of "orientational others" which are internalised during the process of identity formation as a scientist, that is, during professional socialisation. Manford Kuhn [1964] defined the "orientational other" as follows:

i. ". . . the term refers to the others to whom the individual is most fully broadly and basically committed, emotionally and psychologically".

ii. ". . . it refers to the others who have provided him with his categories".

iii. ". . . it refers to the others who have provided and continue to provide him with some of his categories of self and other and with the meaningful roles to which such assignments refer".

iv. ". . . it refers to the others in communicating with whom his self conception is basically sustained and/or changed".

Hill and Howden [1974] recognised the relevance of this definition of Kuhn's to the processional scientist and coined the phrase "professional orientational reference group" to define the "orientational other" in the context of the world of science.

Whereas it is agreed that these are indeed *some* of the features necessary to an explanation of how and what a scientist internalises and maintains as his identity as a scientist, this definition is not broad enough in scope to account for the nature of scientific sub-universes as finite provinces of meaning which are quite strongly separated from non-scientific sub-universes. There is, furthermore, insufficient emphasis on that aspect of the relationship between the individual and others that is based on self justification, or more generally speaking, legitimation. That is, any professional expends some energy in processes aimed towards the continued security of individual or collective interests. This process may often occur in a hostile or strongly competitive environment, and for that reason it may arise that an individual, or group, has a particularly well defined set of arguments and beliefs that justify particular claims on scarce resources, and also ensure the continued or increased status of the activities and individuals involved. For this reason the concept of legitimation has been added to Kuhn's definition of professional orientational other. This concept of legitimation is held to be of fundamental importance to an understanding of the process of scientists' separation between scientifically relevant and non-scientifically relevant work.

Kuhn's concept of orientational reference group can now be redefined as follows:

(i) The term refers to the others to whom the individual is broadly and basically committed.

(ii) The term refers to the others who have provided the individual with his/her epistemological categories *and specialised stocks of knowledge.*

(iii) The term refers to the others who have provided and continue to provide the individual with some of the categories of self and other, and with the meaningful roles to which such assignments refer.

(iv) The term refers to the others in communicating with whom the individual's self conception is basically sustained and/or changed.

(v) *The term refers to the others who provide a legitimation for the scientific work of the individual and who provide some overall definition of the difference between scientifically relevant activity and non-scientifically relevant activity.*

The main functions that the orientational other has are now slightly different to those defined in Hill and Howden [1974]. Characteristics (i) to (iii) relate, respectively, to the provision of the individual with psychological and emotional commitment, an epistemological system, and an institutionalised mode of internalising roles and role definitions, and characteristics (iv) and (v) relate to the social control and role reinforcement of the individual, but (v) specifically relates to the scientist's mode of dealing with processes of legitimation that are directed outside of the context of research.

This orientational other is conceived as functioning in consciousness as a relatively anonymous reference group. That is, although significant others may at times be important to scientists the combination of many such significant others in the context of a network of professional colleagues necessarily functions in a generalising way in consciousness. The professional orientational reference group is, as identified earlier in Section 2.7-1 "more than" a "significant other" but "less than" the "generalised other" defined by Mead.

The main point of departure of this definition of the professional orientational reference group from that proposed in Hill and Howden [1974] concerns the addition of a dimension which specifically demonstrates the orientation of this reference group to aspects of social life other than internally directed activity. Legitimation is the major process that guarantees the integration of any professionalised system within the social mainstream. Thus, point (v) is not merely an extension of point (ii) which is understood to be internally focussed, with the context of research (as defined in Section 2.7-1), nor is it an extension of point (iii) or (iv) which likewise were originally conceived as explaining the integration of the professionalised character structure from the standpoint of the demarcation of professional behaviour and socialisation from other aspects of daily life. The institutional consequences of this process of legitimation will be discussed in the next section.

4.8 The context of legitimation vs. the context of research

As pointed out in the last section the professional scientist is not totally inward looking. That is, scientists do not spend all their waking moments performing research. There are always times when scientists need to interact and communicate with others outside of a usually fairly narrowly defined "context of research", which, as we defined it in 2.7-1, is a sub-universe of meaning dominated by research goals and theoretical structures which provide notions of relevance for the process of research. This process of interaction outside of an "in group" context may often be highly politically and economically motivated, particularly if funding is being sought, or existing research justified.

In this sense the cognitive field of scientists in a scientific research program exists potentially in two modes: a context of research and a context of legitimation. The context of legitimation emerges in situations where scientists are concerned with the justification of their research generally outside of an "in group" context of research. As I will go on to show in Chapter 6, in the case of a highly institutionalised context of legitimation, a coherent set of beliefs may exist as a cognitive structure which is entertained in relative isolation from other cognitive structures associated with a particular research program. Generally speaking, as a legitimating device any belief system functions, as Berger and Luckmann [1967:105] put it, to keep outsiders "out" and insiders "in".

Both the contexts of research and legitimation are scientific "sub-universes" in the sense that they each have a different "accent of reality" or "cognitive style" - as Schutz and Luckmann define these two terms, we are talking about different provinces of meaning - compatible experiences [1974:22-25; see also my Section 2.2]. Structures in the context of legitimation such as a belief system differ from structures in the "context of research" in that they may be at times interpretationally relevant to research, but rarely (and then only indirectly) are they thematically relevant to research. I have termed this "external" context the context of legitimation. Insofar as scientists in the context of legitimation consciously retain their identity as members of a research program (indeed the idea of legitimation requires this) the context of legitimation is a sub-universe of the research program. That is, both contexts are sub-universes of the research program, and movement between contexts will be necessarily associated with some "leap" in consciousness (see Footnote 2.25).

Both contexts of research and legitimation are defined and maintained in processes of interaction - both at the face to face level and also at more anonymous levels. In the case of sub-universes of the world of science the professional orientational reference group is the primary agent that mediates scientific consciousness. As defined in Section 4.6, the professional orientational reference group is partially constituted by "others who provide a legitimation for the scientific work of the individual and who provide some overall definition of the difference between scientifically relevant activities and non-scientifically relevant activities".

This "external" context functions similarly to Habermas' "legitimation system" insofar as generalised motives, or in other words "diffuse mass loyalty" is elicited in this context, but at the same time more political participation in social life is nonetheless generally avoided [cf. Habermas, 1976:36]. It is important to note that the context of legitimation also tends to function similarly for both researcher and non-researcher - to put it in slightly different terms the context of legitimation is, amongst other things, an institutionalised safety valve. As Coser [1972:46] puts it, "the availability of safety valve institutions leads to a displacement of goals in the actor: he need no longer aim at reaching a solution of the unsatisfactory situation, but merely at releasing the tension which arose from it". A belief system in the context of legitimation may function in a similar way. Thus a scientist can, in effect, rest somewhat easy in the knowledge that although his research might have only minuscule potential effect on a perceived social problem, his better intentions at least find some expression in a shared belief system that justifies his research. That is, to put it bluntly, one can "get on with research" and stop worrying about what its social effects might be and what values others will put on the research. In phenomenological terms this process may be described as the "bracketing" [19] of social considerations as "external" to the research process. That is, if social considerations emerge during the process of research they are in effect given a different value to those theoretical and technical concerns which have the scientist's first priority at that moment. In this process these social issues are actually set aside as issues that may, in principle at least, receive further attention at some future time in a context other than that of research.

Tension release may be one consequence of a scientist's ability to move between these two sub-universes, but we have no reason to believe that the sub-universe of the research program is as a consequence, tension free. The existence of these two modes of thought and action are after all, an expression of the different needs and priorities engendered by different social demands. These different needs and priorities may in fact equate to a situation of conflict - and, of course, safety valves do not eliminate the forces that give rise to problematic situations. In other words, the institutionalisation of these two contexts need not eliminate the possibility of scientists experiencing conflict at some level of consciousness. Scientists may, for example, experience conflict about the different criteria involved in performing socially useful research as opposed to scientifically respectable research. This obviously possible conflict situation could be described as a "double bind" situation, somewhat parallel to the schizmogenetic double bind situation Gregory Bateson observed in the experiences of many children.

In Gregory Bateson's well known "double bind theory of schizophrenia" one consequence of recurrent conflicting communication in a family context was "schizophrenia" in a child. The essence of Bateson's [1956] theory (which considerably predates Laing's version of the theory) is that when an individual is caught in a situation of having to continually reconcile contradictory injunctions (and Bateson focuses on the mother as the most powerful source of conflicting messages) coping mechanisms which are socially unacceptable may result (i.e. "schizophrenia"). Bateson's focus is on primary socialisation and face-to-face communication, but the model he sets up is analogous with the situation described above even though our focus is on secondary socialisation and communication mostly with relatively anonymous reference groups (i.e. an institutional rather than individual focus).

The main difference between the situation Bateson observed amongst children and the situation one encounters in the scientific life world is that the alternation of scientists between a context of research and a context of legitimation is not generally perceived as an unacceptable response to a problematic situation - it is nonetheless, socially unacceptable insofar as research is made insensitive and unresponsive to social needs and demands. Nor is it as confusing or emotionally unsatisfactory from an individual perspective. By virtue of this mechanism of escape a scientist is able to continue to function quite effectively as a socially productive individual. The "schizophrenic" child is on the other hand, usually considered as socially worthless, and is not usually capable of a relatively controlled alternation between psychosis and some more acceptable mode in conscious response to particular situations. In comparison the scientist is generally in fairly tight control of his alternations between "scientifically relevant" and the "scientifically relevant" sub-universes.

Both situations are nonetheless mechanisms of coping with problematic situations, and if we can see through the different social valuations afforded to each, they are equivalent in providing emotional and cognitive release for the individual. Neither situation is entirely satisfactory from the perspective of individual interest. The "schizophrenic" child is generally a confused and unhappy individual and although the institutionalisation of scientists' modes of dealing with conflict situations may make life quite bearable there may still be some level of conflict that is not resolved through institutionalised mechanisms of coping. Indeed, as suggested above, the lack of integration of different aspects of scientific life (such as research and legitimation) may even create conflict. We will explore this situation further in the first case study.

Finally, the idea of legitimation has definite implications with respect to "ideology". Although I have avoided the typical Marxian usage of ideology whereby epistemological judgements of "error", "falsity", "distortion", "inversion", etc., are involved, the context of legitimation is still obviously concerned with the interests of a particular social group (such as a team of researchers, a university department, etc.) and in this sense the context of legitimation is ideological in function. In addition however, I take it for granted that in a bourgeois social epoch the world of science is not excluded from what Berger and Pullberg [op.cit.] described as the *de facto* broken unity between the act of producing and its products. Ideological processes involve a separation in consciousness and action between the "reality" of day to day practices and the meaning (and justification) attached to these practices. In this sense the context of legitimation is an indication of the way that the natural sciences are ideological.

4.9 Summary, conclusions and hypotheses

Chapter 4 completes the theoretical basis for the case studies which follow in Chapters 6 and 7. In this chapter the general nature of the goals of scientists have been explored in considerable detail - viz, the relationship between the goals of scientists and intentionality of consciousness, projects of action, reference groups, and the institutionalisation of research programs was developed. One of the basic points made in this chapter was that the instrumental rationality of both the natural sciences and everyday life tends to be glossed over in sociology - probably because instrumentalism is so widely accepted as a necessary aspect of all pragmatically motivated behaviour. As a consequence this chapter was intended to re-open discussion about the constitution, in action, of modern day consciousness, and to deepen understanding about the constitution, in action, of scientific consciousness.

As the final section of the opening theoretical section of this thesis, Chapter 4 represents a partial synthesis of many of the concepts advanced in the earlier chapters. In fact, a number of general hypotheses can now be shown as emerging at particular points in the development of this chapter. These hypotheses are intended as a summary of the material presented so far and insofar as they do embrace a wide range of material much of which has been discussed in the first three chapters they were not introduced in the text of this chapter since they would have been diversionary at that stage. They do however, flow fairly spontaneously from the context of Chapter 4 and will be listed below in conjunction with a summary of the main points raised in this chapter.

Given the breadth of the first four chapters a very large number of hypotheses could, in principle, be elaborated, but this would not necessarily provide the clearest foundation for further investigation. What I have attempted to provide therefore, are a number of propositions that are both central to the argument that has been developed so far and which can at the same time provide a fruitful basis for empirical research. As mentioned, these hypotheses can be regarded as a summary of the theoretical framework that has been developed so far. This is not meant, though, to imply that the theory that has been developed is purely speculative. Rather, we have merely reached a point in our explorations where theory needs to be "grounded" in empirical research that can adequately test and develop ideas that, as yet, do not have a firm basis in research.

In this chapter it was suggested that the natural sciences provide a paradigm case of goal directed behaviour - goal rationality being a dominant form of rationality in modern times. We do not yet have however, empirical information which shows the precise manifestation of instrumentalism in research.

Hypothesis: Scientific research is predominantly instrumental by virtue of being more highly directed towards technical goals and the means for their realisation than towards questions about the value of these goals.

The nature of goal directed behaviour in the natural sciences has been explored in this chapter through an integration of phenomenological, symbolic interactionist and structuralist perspectives. On the basis largely of the work of Alfred Schutz, goals have been conceptualised as objectifications of the "in-order-to motive" of action. The first premise of goal orientation is the intentionality of consciousness - that is, as emphasised in phenomenology, all our cogitations refer to objects in consciousness that are, in their essence, meaningful. Thus, goals as objects of consciousness were described as existing as subject-in-relationship-to-the-object and object-in-relationship-to-the-subject. The social nature of the mode of the intentionality of goals in scientific research formed the subject of the remainder of this chapter.

Scientific research was described in this chapter as projects of action within the framework of a research program, or in other words, the research program was redefined as the institutionalised form of thematically, motivationally and interpretationally related projects of action. These structures of relevance provide at different levels a context of shared meaning for individual scientist's research activity. The following hypotheses deal with the basic institutional structures that provide the institutional context for scientific research.

Hypotheses: *Scientific research occurs in the context of a structured cognitive field which consists of interpenetrating levels: metaphysical, theoretical, subject concern and technical levels.*

Cognitive structures in the context of research provides motivational, thematic and interpretational relevancies for research.

Most scientists perform research as part of a research program which is constituted through the collective activities of a *group of research workers who share a commitment to particular research practices and techniques, who are directed in their research towards a shared set of goals, and who share, to some extent, a common stock of specialised knowledge.*

Scientists are directed in their research towards a wide range of goals which span different levels of the cognitive field of a research program.

The concept of project that was introduced in this chapter incorporated a concept of goal since a defining feature of action undertaken in programs of research is that such action is to some extent devised in advance. On that basis goals were defined in phenomenological terms as objectifications of the "in-order-to motive" of action. In these terms, goals are a necessary condition for the functioning of consciousness. What is not necessary however, is the contemporary mode of institutionalisation of the processes of formation, evolution and achievement of goals - given the apparent reification of many aspects of social life in a capitalist social system it was postulated that the goals of scientists may often be reifications rather than freely evolving objectifications of creative human potential. In this condition the institutionalised context of individual research stands between the individual and his desired projects.

The following hypotheses do not focus specifically on alienation in science but insofar as they deal with the potential for change in research programs and some of the ways in which scientists come to terms with the social, political and economic context of research the hypotheses are basic to any consideration of alienation. The hypotheses are particularly significant in their attempt to clearly describe aspects of the scientific life world that tend to be presupposed on the basis of no well researched empirical basis.

Hypotheses: *Not all the goals that are perceived by scientists to be relevant to their research remain equally relevant.*

The research goals of scientists change over time.

Scientists tend to bracket social considerations about their research as "external" to the research process.

Scientific research varies in its orientation towards social application.

"Practice oriented" research is more highly constrained by social, economic and political factors than is "basic" research.

Central to the discussion of the institutionalisation of goal orientation developed in this chapter was a concept of *professionalism* derived from the work of Terence Johnson. This concept was developed as a way of accounting for the prevailing system of social control within science. The concept was seen to be broadly useful in understanding the processes of institutionalisation of scientific consciousness, but Johnson's concept of "collegiate control", and indeed all his other related concepts did not provide a fully adequate theoretical basis for an understanding of professional socialisation and professionalised "universe maintenance". Towards that end the concept of "professional orientation reference group" was developed as the major reference group which mediates between individual consciousness and shared meanings within science. Through the agency of the professional orientational reference group a structured cognitive field is generated and supported in individual scientific consciousness.

Hypothesis: *Scientists are subject to the social and cognitive control of professionalism which operates through the agency of professional orientational reference groups.*

The concept of professional orientational reference group was predicated in this chapter on the existence of processes of legitimation that are necessary for the preservation of professional identity. In practice most scientists distinguish between their research and the utility and social consequences of their research. This separation reflects a distinction between two sub-universes of meaning within the research program - the context of research as opposed to the context of legitimation. The theoretical basis for these two contexts have been further developed in the chapter in terms of their nature as sub-universes of meaning, alternation between which provides tension release - that is the concept of legitimation was described as having the function of an institutionalised safety valve. The institutionalisation of these two contexts within a research program may not be sufficient to eliminate all tension, however; indeed, such processes may actually reify existing conflicts of relevance. This latter subject was briefly discussed in terms of Gregory Bateson's "double bind theory of schizophrenia".

Hypotheses: *Professional orientational reference groups provide a basis for scientists' distinctions between and definitions of, scientific and non-scientific activity.*

Scientists move in thought and action between two sub-universes of meaning; a context of research and a context of *legitimation.*

This movement between sub-universes may engender in scientists a conflict of relevancies.

4.9-1 Summary of hypotheses

In this section the hypotheses that were presented above have been re-ordered to show a clearer logical development (that is to say, a logic of development that is not over-constrained by the order of development of Chapter 4). The hypotheses have been numbered for future reference and also referenced to sections where relevant concepts and definitions were first presented, or where they have been discussed at some length. In addition, four more hypotheses have been added. These last hypotheses were not explicit in the first four chapters but are relatively clear implications of the theory developed there. They are necessary for the facilitation of a comparative analysis of the two case studies that follow. Accordingly, the hypotheses have also been labelled as "general hypotheses" and "hypotheses specifically oriented towards the comparison of research programs".

(a) General hypotheses*

1. Scientists are subject to the social and cognitive control of professionalism which operates through the agency of professional orientational reference groups (see Sections 2.4 and 4.7).

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* Note that these hypotheses are restricted to the physical sciences and physical scientists.

2. Professional orientational reference groups provide a basis for scientist's distinctions between and definitions of, scientific and non-scientific activity (4.7).
3. Scientists tend to bracket social considerations about their research as "external" to the research process (2.4, 3.1, 4.8).
4. Scientists move in thought and action between two sub-universes of meaning; a context of research and a context of legitimation (2.7-1, 2.7-2, 4.8).
5. This movement between sub-universes may engender in scientists a conflict of relevancies (4.8).
6. Scientific research occurs in the context of a structured cognitive field which consists of interpenetrating levels: metaphysical, theoretical, subject concern, and technical levels (2.7-2).
7. Cognitive structures in the context of research provide structures of relevance for scientists' research (2.5).
8. Scientists are directed in their research towards a wide range of goals which span different levels of the cognitive field of a research program (2.7, 4.6, 4.2).
9. Not all the goals that are perceived by scientists to be relevant to their research remain equally relevant (4.2, 4.6).
10. The research goals of scientists change over time (4.6).
11. Scientific research is predominantly instrumental by virtue of being more highly directed towards technical goals and the means for their realisation than towards questions about the value of these goals (1, 4.6).

12. Most scientists perform research as part of a research program which constituted through the collective activities of a group of research workers who share a commitment to particular research practices and techniques, who are directed in their research towards a shared set of goals, and who share, to some extent, a common stock of specialised knowledge (2.2).

(b) <u>Hypotheses specifically oriented towards the comparison of research programs</u>

13. Scientific research varies in its orientation towards social application (2.6).
14. "Practice oriented" research is more highly constrained by social, economic and political factors than is "basic research" (2.6).
15. The level of institutionalisation of the context of legitimation of a research program is positively correlated with the level of scientific marginality of a research program (2.5, 4.8).
16. The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated with the level of orientation of program members towards the more technical goals of the program (4.8).
17. The level of institutionalisation of the context of legitimation of a scientific research program is negatively correlated with the level of orientation of program members towards higher level goals (4.8).
18. The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated with the level of practice orientation of research in the program (3.1).

FOOTNOTES TO CHAPTER 4

1. This is, in fact, how Habermas [1971:91] defines *work*, which he contrasts with interaction. Whilst Habermas' diagnosis of the bourgeois epoch as one which is dominated by instrumentalism (particularly at the political level) is consistent with the argument advanced in this thesis, I am not attempting to pursue a distinction between work and interaction. In fact, as will become clear the interpretation to be given to research, as a type of work, is inseparable from notions of interaction implicit in the use to be made of reference group theory. I would add that Habermas does not direct his attention to the precise implications of the instrumentalism of the physical sciences which he identifies in Knowledge and Human Interests - or certainly not in a way that is of great use to a practical sociology of scientific knowledge.

2. For example, Cohen's [1975:69-94] account of the assumptions of the theory of action presupposes that we already know what a goal is. This common-sense approach to goals is also noticeable in Marxist literature on action. The concept of praxis, for example, deals with action at a level that has little bearing on the phenomenology of goal orientation.

   Even amongst organisation theorists who as a group have devoted considerable attention to various aspects of goals the level of theoretical penetration does not appear to be high. Within this literature there are a number of distinct traditions, including studies of the range of goals pursued by industrial firms, the evolution of goals within organisations, and the means of changing goals in normative institutions. Nonetheless, the understanding of what a goal is still tends to remain at the level of "common-sense".

   For example, see R.M. Cyert and J.G. March, A Behavioural Theory of the Firm, Prentice-Hall, Englewood Cliffs, N.J., 1963; D.L. Sills, The Volunteers: Means and Ends in a National Organisation, Free Press, Glencoe, Ill., 1957; and C. Perrow, "The Analysis of Goals in Complex Organisations", American Sociological Review, 26 [1961], pp.854-866, respectively.

3. See, for example, P. Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason", Social Science Information, 14 [1975], pp.19-47; also R. Whitley [1976, op.cit] and "The Structure of Scientific Disciplines and their Elites", in G. Fourez and J.F. Malherbe (eds.), The Stakes of Scientific Professional Training, Presses Universities de Narmur, Belguim, 1975.

4. For example, R.D. Whitley, "The Sociology of Scientific Work and the History of Scientific Development", in S. Blume (ed.), New Perspectives in the Sociology of Science, Macmillan, London, 1977. See also Sections 2.4 and 2.4-1 of this thesis.

5. R.D. Whitley [1975:41].

6. N.C. Mullins [1972:52, op.cit].

7. These two examples are drawn from W. van den Daele et.al., [1977:12, op.cit].

8. The shaping of knowledge by these forces has been clearly demonstrated by P. Forman [1971:note 11, op.cit] and R.M. Young [1969 and 1971:note 11, op.cit], respectively.

9. It is noteworthy that accounts of science written in a phenomenological tradition appear to owe more to the philosophy of science than to the sociology or history of science, for example, Gurwitch [1974]; Luckmann [1973]; and Kisiel [1973].

10. Intentionality, that is, "being conscious of . . ." [Schutz, 1970:146] is thus a very general property of consciousness. We should note that the way "intending" is normally used as "having a purpose" effectively narrows this definition to an interest in the attainment of our purposes at hand [cf. Douglas, 1973:26: "human thought, then, is fundamentally oriented towards usefulness, towards doing things that have the affects we intend"]. Whereas this is true for the "natural attitude", as Schutz defines it, this mundane pragmatism is not so directly relevant to all modes of consciousness, such as heightened states and dreaming. But, as Schutz [1971:15] describes our everyday being-in-the-world, intentionality certainly boils down to a pragmatically based interest in the motives of others:

> "I am interested above all not in the overt behaviour of others, but in their intentions, and that means in the in-order-to motives for the sake of which, and in the because motives based on which, they act as they do.

11. These issues will be taken up later in this chapter.

12. The subject of autonomy and science is dealt with at greater length in Hill and Jagtenberg [1977:Ch. 3]. Polanyi and Popper have both been used by others as a basis for conservative arguments about the importance of scientists' autonomy.

13. "One experiences that which is taken for granted as a kernel of determinate and straightforward content to which is cogiven a horizon which is indeterminate and consequently not given with the same straightforwardness. This horizon, however, is experienced at the same time as fundamentally determinable, as capable of explication" [Schutz and Luckmann, 1974:9].

14. This sense of reification is not well developed in the phenomenological tradition, although the concept is not entirely absent from phenomenology. For example, Husserl distinguishes between "active" and "passive" synthesis in consciousness. "Passivity in general is the realm of things that are bound together and melt into one another associatively . . . without any of the self evidence of original activity" [Husserl, 1978:51]. This form of activity is opposed to the active engagement of consciousness which is marked by "explication", "articulation", and the reactivation of what were passive meaning structures. In this latter process real communalisation of knowledge becomes possible [for example, cf. Husserl, 1978:54, 55].

15. In Kuhnian terms, "normal science".

16. Note that the term "actor" is not necessarily a single person. It may refer to collectivities of various kinds, for example, research programs and institutions [Schutz and Luckmann, 1973].

17. It is important to note that goals (an aspect of motivational relevancy) only make sense in the context of other types of relevance (for example, thematic or interpretational relevance). In the case of a highly institutionalised cognitive domain such as one might expect to find in the natural sciences, one can speak of shared and interrelated cognitive structures (for example, theoretical knowledge which provides a disciplinary and specialty "background", or structures of goals) which constitute an institutionally defined cognitive field within which and through which an individual scientist will perform research (cf. Section 2.7-2).

18. One is never, of course, completely free to choose one's future. In other words, one's future actions are necessarily predisposed in particular directions. At a cognitive level it is suggested that these "relative condensations of meanings" correspond to a cognitive field that is constrained, or "problematic". The attendant narrowing of choices is the result of particular social and historical circumstances. The extent to which one may be conscious of these predispositions is another question.

19. "The idea of bracketing is part of the methodological foundation of phenomenology: by bracketing the objective world we give it a different value" (Husserl, quoted in Schmitt [1967:59] - Husserl originally derived the term from mathematics, where an expression can be placed in brackets and preceded by a + or - sign). The term is widely used as a way of characterising the transcendental phenomenological epoche, wherein "the natural belief in the existence of what I experience" is "invalidated", "inhibited", "disqualified" [Husserl, op.cit], in order to look at the world through "new eyes" directed towards essence and transcendental subjectivity (and intersubjectivity). The idea of bracketing has been used to characterise epoches other than that of transcendental reduction, however. For example, Schutz and Luckmann [1974:27] use the term to characterise the way that doubts about the existence of the outer world are suspended in the "natural attitude" of everyday live. Berger and Luckmann [1967:25, 34] have translated the term into a methodological prescription for the sociology of knowledge and any empirical sociology: epistemological questions must be placed inside "phenomenological brackets" and raised as a methodological problem in philosophy "which is by definition other than sociology". This latter sense of bracketing is the sense I would use to characterise the attitude of most natural scientists to the issue of the role of social, political and economic factors in the process of research and constitution of scientific knowledge.

CHAPTER 5: METHODOLOGY

"Waste not your time, so fast it flies;
Method will teach you time to win;
Hence, my young friend, I would advise,
With college logic to begin.
Then will your mind be so well braced,
In Spanish boots so tightly laced,
That on 'twill circumspectly creep,
Thought's beaten track securely keep,
Nor will it, ignis-fatuus like,
Into the path of error strike".

Goethe, Faust, Pt. 1.

5.1 Introduction

This thesis has been designed as a theoretical and empirical exploration of aspects of the natural sciences. The case studies which follow have been developed as a way of empirically "grounding" and further developing theory which has provided a starting point for the analysis that has been conducted. The methods that have been employed in this theoretical and empirical exploration have a general similarity with most "scientific" endeavours in that theory (amongst other resources) has been used to develop hypotheses which have been tested against empirical data - that is, like any scientist I have been concerned to see whether the theories and hypotheses that have been developed do actually "fit" reality.

Nonetheless, some qualifying remarks about the nature of sociology as science are necessary. Sociology is an activity which deals with the interpretation, creation and recreation of social reality by historically located individual acting subjects. At the same time however, sociologists depend on a reality principle predicated on the existence of an external objective world which individuals share and to some extent, consensually validate as "real". That is, sociology has unavoidably, a subjective dimension which is necessarily constitutive of social theory and research. This I take to be a fundamental postulate of any "interpretive" sociology of a kind that derives some inspiration, as I do in this thesis, from the orientations of Schutz, Mannheim and Max Weber to subjective meaning.

As a consequence of this fact that sociology is necessarily involved both with an "expressive" and "evidential" level of meaning (see Section 2.1) it is not therefore, possible to "test" *sociological* propositions by the exact same criteria employed by natural scientists in their speculations about an external, objective, non-self-conscious reality - natural sciences still tending to provide a paradigm for "scientific method". For example, it may not be possible to "control" variables, repeat "experiments" or construct "critical tests" of hypotheses. The methodological implications of the differences between the cultural (or "human") sciences and the natural sciences have been well discussed in the methodological literature [see, for example, Bell and Newby, 1977; Glaser and Strauss, 1968; Madge, 1967; and Goode and Hatt, 1952] and do not need further elaboration here save an explanation of the methodological consequences of this difference that are important in this thesis.

Now, although the above remarks are, in a sense, "general sociology" and perhaps capable of assumption without further comment, I raise them simply because the methodological implications of such a stance are not reflected in most empirical sociology which still proceeds as if it were possible to be entirely "objective" in a "properly" scientific fashion. In this thesis a highly important methodological principle is that the "reality" of shared structures of meaning is context dependent - that is to say, in understanding social reality we need, necessarily, to have the widest possible knowledge of the social context of individual action. Any particular hypothesis which might seek to explain or predict aspects of social reality can then only make sense sociologically if the institutional context is well known, if the nature of the individual's consciousness of his social environment is well known, and if the broad historical patterns which give ultimate sense to our efforts to be scientific are well known - these requirements derive directly from Mannheim's [1952] reflections on social reality, as discussed in Chapter 2. An important consequence of the necessarily contextual nature of sociology is that any particular hypothesis will become extremely complex, given the amount of information that is needed to make full sense of all its component concepts. Or in other words, the predictive power of sociological hypotheses is necessarily diluted by their complexity and different levels of meaning. For this reason the hypotheses that I have developed in this thesis have been postulated as complexes of many dependent variables and no attempt has been made to "separate variables" in order to "test" their individual impact on independent variables. Rather, the hypotheses advanced have been evaluated on broadly qualitative and quantitative grounds as "fitting" or "not fitting" the evidence. That is to say, there is absolutely no reason why any of the data presented could not or should not be *reinterpreted* in the light of new evidence. All efforts have been made to work logically and rationally, but it must be stressed that the primary purpose of this thesis is not the prediction or management of scientific behaviour. My main purpose is one of understanding and consequently, I make no apology for the apparent complexity of the hypotheses that will be developed later in this chapter. Complexity is, after all, only the consequence of the analytic moment of science which divides the world into many component parts. But complexity is necessarily bound to simplicity as the synthetic moment of science when all the parts are integrated into subjectively meaningful patterns. This latter simplicity is the ultimate purpose of this thesis.

Nonetheless, consistent with the basic principles of any "scientific" method as presented in most methodology texts (whatever the discipline) I take it for granted that sociology, despite its cultural nature, is nonetheless concerned with the testability of hypotheses and the validity and reliability of concepts and data. These concerns are basic to the design which follows.

5.2 Aspects of the historical background of the techniques employed in the fieldwork

When the field research for this thesis was commenced in early 1977, there was not a great deal of methodologically useful material available in the sociology of science literature. Given the fairly nascent state of more cognitively oriented sociology of science at the time this relative absence of practical guidelines was hardly surprising, of course. The fieldwork was begun, therefore, in an "open" fashion. That is, I read what relevant material I could find, found very little that satisfactorily told me what to do, had a few general discussions with colleagues, and then plunged in, trusting to the dialectic nature of research - sink or swim indeed! The situation was probably not as chaotic as I first thought, however, for methodological patterns rose to the surface fairly quickly - the basic outline of the research design was settled at roughly the time that exploratory discussions were being held with group members, and except for the elaboration of details remained essentially constant throughout the research.

Although there were, in 1977, no coherent guidelines for a cognitively oriented sociology of science the existing literature in the sociology of science was of some use insofar as many of the problems experienced by sociologists of science at the time were of general relevance to a cognitively oriented sociology of science. On the basis of this literature (but particularly the work of Michael Mulkay, David Edge and Stephen Hill) and the problems I had already experienced in the field, a number of problems that will confront any cognitively oriented sociologist of science can be listed. These problems are still highly pertinent today, and for that reason I have written them in the present tense.

All of these points are reflected in Michael Mulkay's [1976]

"Methodology in the Sociology of Science: Some Reflections on the Study of Radio Astronomy". This paper is, in fact, well worth reviewing in detail for it provides an introduction both to the kinds of problems that confronted me at the time and to the kind of approach adopted in response to these problems, as well as providing a useful point of reflection for further discussions about appropriate methodology.

To begin with, a more cognitively oriented sociology of science is not well developed in that:

(i) There are no well established methodological guidelines for a sociology of scientific development (that is, a sociology which deals with cognitive as well as social aspects of scientific development); and

(ii) There are not many available case studies that can be used as a basis for the adequate testing of more cognitively oriented research findings.

Then there are two highly important, but often neglected, general points about practical research:

(iii) Any sociologically defined phenomenon is operationally definable by a number of indicators and therefore the selection of the "best" indicators to use may be problematic; and

(iv) It is necessary to allow for the fact that a research account is the product of interaction between researcher and respondent.

Finally, there are a number of problems that are specific to research with natural scientists:

(v) It may be difficult for a sociologist to establish rapport with scientists;

(vi) Scientists may be at least partially mistaken or biased about historical events;

(vii) There may be a confusion in scientists' accounts between historical accuracy and scientific accuracy; and

(viii) Scientists may be uncritical about a researcher's account of scientific development, even if the scientist is directly involved.

In general, Mulkay's paper is written with a kind of optimistic uncertainty fitting to any researcher who, having found himself in a new area of research equipped with little more than good advice from the literature was able to generate a large amount of valuable and well received information about the evolution of a scientific research area:

> "In the sociology of scientific development, because it is a fairly new area of detailed enquiry, we have the opportunity of constructing explicit methodological theories more or less from the start and of conducting future research so as to improve these theories. For some time, of course, there will be a high degree of uncertainty, because it is at present impossible to assume that any research techniques produce results which can be reliably interpreted" [p.219].

Mulkay's uncertainty partially stems from the realisation that sociological research is a social act, "an act in which those being studied usually participate with the investigator to produce the final observations" [p.207]. This means that the sociologist is faced with a problem regarding the reliability and validity of his findings. For example,

> "The subjects under investigation are likely to be responding to a variety of definitions of the investigator . . . any one sociologically defined phenomenon can be observed by means of several indicators . . . these indicators must be interpreted in relation to the specific social contexts created in the course of research" [p.208].

Consequently,

> "As a result of considerations such as these, we decided that our study should be frankly exploratory. In other words, we decided that we would not define in advance the detailed questions which were to be answered nor the precise research procedures that were to be adopted" [p.209].

In a parallel way, my own awareness of the complexity of the task of generating reliable research accounts (with limited resources) forced me to approach my research as primarily exploratory.

Mulkay had some very different practical considerations, however. He was a sociologist and his co-worker, David Edge, was an ex-radio astronomer (and newly a historian of science) - how were they both going to relate to their scientist subjects in such a way as to gain their confidence and generate reliable information? "If we are to study in detail the operation of scientific communities we must have the active co-operation of participants or ex-participants" [p.210, 211]. Mulkay and Edge solved this problem by commencing interviews (and presumably the interaction process) with questions from the ex-scientist. In this way the ex-scientist who had been accepted as sufficiently scientifically completent to conduct an intelligent conversation about radio-astronomy was able to establish rapport with the respondents. Once rapport had been established the sociologist was able to become involved, but in interview situations where there were both sociologist and ex-scientist present the sociologist only entered the conversation when the respondent's account of social events appears in some way problematic - at most other times he tended to be an outsider. The third kind of interview situation that Mulkay and Edge used was interviews conducted by a sociologist alone. These interviews tended to have a low level of technical consent:

> "Even when the sociologist has a good layman's knowledge of the field, he is unable to discuss technical issues with the flexibility of a participant or ex-participant. Yet technical and social issues are intimately related. A

> scientist's typical account of why he took up a particular line of research at a particular time will stress technical considerations . . . Consequently, if one wants to know about the effect of competitive or other social pressures on the decision, it is helpful to be able to enter into a dialogue regarding technical factors in the course of which the respondent can be guided towards greater consideration of social factors. It is therefore useful to have a technically competent interviewer present" [p.214].

Clearly, one of the problems encountered by Edge and Mulkay related to the separation of their competences. The process of interviewing a number of respondents any more than once doubtless proved complex and taxing for all parties involved. This was, fortunately, less of a problem in my own research. Having had both a scientific/engineering and a sociological background I was able, to some extent, to combine the roles of scientist and sociologist. On meeting the chosen scientists for the first time I would stress my own scientific background (Honours level in chemical and fuel engineering and at one time, a professional officer in a Chemistry School) and attempt to begin a process of clarification of my own understanding of what the scientist's research was about. These technical conversations required a fair amount of technical research on my part, but usually providing I had some knowledge of the general principles involved a scientist would be only too pleased to fill in the details. That is, at best I was able to establish rapport on the basis of my being an interested ex-scientist/engineer. My role as sociologist was, as a consequence, relatively easy to play up or down as the situation required. More socially oriented lines of questioning were able therefore, to be introduced in the normal course of a two person conversation. It must be confessed, however, that I often found myself in deep scientific matter, and furthermore, never fully lost the tag of "sociologist from Wollongong".

Hill [1970] relates a similar situation in his "Swimming with Sharks: Techniques of a Multimethod Approach to Concept Validation" where he describes his involvement with several groups of scientists as "participant-as-observer",

> "I maintained the status of investigator to the subjects but sought involvement through social participation with them . . ." [p.300].
> "The participant-as-observer role was all the more appropriate to this study of scientists because my own background included a research degree in physical chemistry. 'Learning the language' in the initial stages of each study hence was not a serious problem; in fact, this ability provided a ready path to my identification with the scientists I was studying" [p.325].

"Learning the language" was a means to rather different ends in Hill's case, however. Because his study was oriented to the values of the scientists he was studying, Hill's interest in his informant's research was apparently primarily directed towards the maintenance of rapport rather than the generation of research accounts. "Swimming with Sharks" is, in fact, a good example of how, despite very similar backgrounds (young male sociologists with some training in the physical sciences), the kind of relationship established with informants will vary according to the interests of the sociologist. Thus, Hill's primary interest in scientists' behaviour rather than their research per se led him to partially conceal his motives. Hill was forced to use his knowledge of "group sociology" in order to protect the validity of his findings and facilitate rapport. Thus, for example, the sociologist was ostensibly "studying management of research through the eyes of scientists" as a "chemist interested in management" [p.326], and "the groups did not realise they were under continual observation" [p.326]. Such tactics were not necessary in my own research, where efforts to minimise the effects of the observer on the behaviour of the informants were not necessary. Thus although, following Gold's [1958] typology, [1] "participant-as-observer" is broadly descriptive of both my own relationship to my informants and that of Hill's relationship to his

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1. According to Gold (and as developed by Babchuk) the relationship between researcher and respondent will fall along a continuum marked by the range "complete participant" through "participant-as-observer", "observer-as-participant", and "complete observer". See Raymond L. Gold, "Roles in Sociological Field Observations", <u>Social Forces</u>, Vol. 36 [1958], pp.217-223; and Nicholas Babchuk, "The Role of the Researcher as Participant Observer and Participant-as-Observer in the Field Situation", <u>Human Organisation</u>, Vol. 21 [1962], pp.225-229.

informants there were significant differences in the openness possible to the sociologist during his research. The methodological basis for this openness will be explained in subsequent sections.

Some of the practical measures adopted by Edge and Mulkay have been already described (i.e. the interviewing procedure). In addition, there were two other tactics adopted that are of relevance to the case work that will be described. Firstly, Edge and Mulkay attempted to deal with the scientifically generated "myth" about Jansky (a pioneer in radio-astronomy) by collecting data from a variety of sources (interviews and the literature). Secondly, the complexity of their subject matter and the "imponderables involved in even the simplest kind of interaction" led Edge and Mulkay to circulate a first draft of their research report amongst their respondents for comment. Both of these tactics were adopted in my own research. In order to more adequately assess the more "mythological" beliefs of my respondents (that is, beliefs at a more "metaphysical" level, and beliefs held in a context of legitimation), I found it necessary to both read widely and interview a range of scientists, including members of the research program, and other individuals with similar specialist interests. This broad range of interviewing was not intended as a check on the content of the research accounts (since unlike Edge and Mulkay's work, my research accounts were of a much less public and "historical" nature). The program of interviewing was designed more to provide a wide context of understanding of the research programs being investigated. And parallel with Edge and Mulkay, in order to check on the accuracy of my reconstructions of scientists' research I circulated drafts of my work. There is a problem involved here though, because if scientists are uncritical about accounts of their research, the circulation of a draft report for comment may be a slightly pointless exercise apart from validating the idea that scientists may in certain circumstances be highly uncritical. In my own research this problem (in conjunction with the more general methodological considerations listed above) led to the development of a re-iterative methodological technique, the "method of repeated feedback, which will be discussed in Section 5.4.

One of the limitations of Edge and Mulkay's work is that information generating interaction with scientists was very largely restricted to interview situations. Given that the focus of their research was largely historical this is to be expected - but even if more informal contact was possible this would not have altered the fact that past events must necessarily be considered beyond the interference of researchers. That is to say, Edge and Mulkay were not physically present during the events they were researching. In terms of Gold's typology which was introduced earlier, Edge and Mulkay adopted the role of "observer". This is of course a situation that many sociologists prefer to relate to; furthermore, it is often unavoidable. Nonetheless, sociological research need not always occur outside of the test-tube, as it were, nor after the event. One of the obvious advantages of actually being present during the research one is investigating is that the scientists concerned tend to relate more strongly and honestly about their research since the sociologist (whoever) was there too. Therefore, some efforts were made in my research to gather data as it was occurring. That is, my interactions with the scientists in the two research programs being investigated was not completely structured around interviews. Rather, from time to time, extended periods of time were spent in the scientists' laboratories and tea-rooms. The amount of useful data actually gathered in these non-interview situations was not in fact great compared with the interviews, but a consequence of one's becoming more of a familiar sight in the laboratory is an enhanced interaction during interviews. In other words, the researcher who merely floats through his repondent's work place may not obtain as much co-operation as the researcher who demonstrates more than a superficial interest in day-to-day events and the life worlds of his respondents. These considerations are fundamental to what one might refer to as "good ethnography" [cf. Law, 1974].

5.3 On the reality of the "research program"

One of the consequences of this lack of relevant methodological guidelines and case studies that was discussed in the last section is that the validation of central concepts is more difficult than would be the case in a specialty where there was some consensus over what phenomena were central and what indicators best preserved the reality of these phenomena. In the sociology of science however, there is not even a working consensus over what scientific collectivities, or units of organisation, are "real" [cf. Section 2.2]. Since this is a problem of considerable importance to the present work, a little more should be said in the defence of the concept of "research program" which will be central to the organisation of empirical material in the case studies.

Woolgar [1976] has written about some of the problems involved in the identification and definition of scientific collectivities. As he points out, any research network is defined in terms of a relative concentration of "interest ties", and as such has no inherent boundary and no absolute criterion of network membership. These kinds of considerations have been partially dealt with in Section 2.2, but a few further remarks are in order since Woolgar's paper does raise questions about the validity of the concepts of "research program", "program membership", and "core group", which I have employed in my case work. The main point to be made is that if researchers would concentrate more on "doing good ethnography" and less on citation analyses the practical need for collectivities which are meaningful to their members would be more fully appreciated. This is not to say that a "research program" or a "core group" are "things" in the minds of scientists (or sociologists). They do however, enable the sociologist to make sense of the relationships between scientists. Furthermore, collectivities based on ethnographic research provide a firm basis for continued interaction with scientists for they certainly have firm ideas about who was more or less involved in their day-to-day research and what the immediate sources of their theory and techniques are. The basis for my own usage of the concepts "program membership" and "core group" follows below.

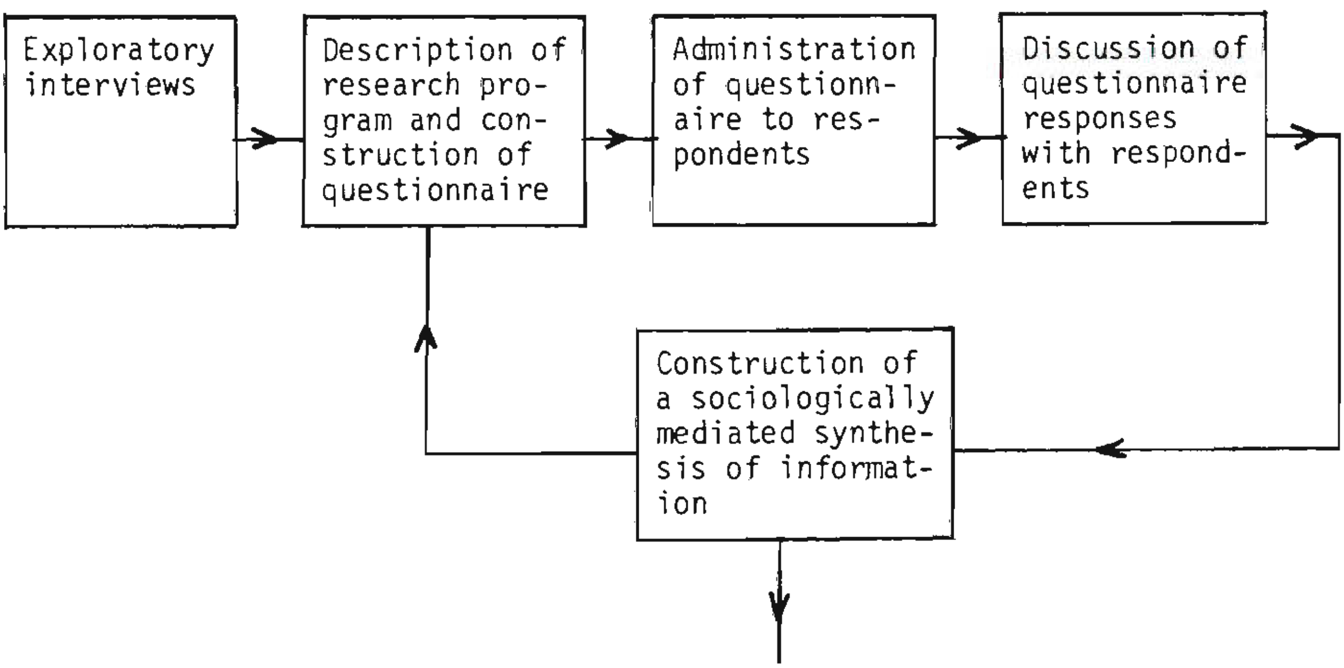
Those scientists who became closely involved with the goals of the program have been designated in this paper as "program members". More generally speaking, the main criterion of program "membership" was a practical commitment to the activities of a group of people working towards shared goals. That is, there were in existence groups of researchers who were involved in a functional division of labour and who shared a common stock of specialised knowledge. Nonetheless, there were in both the research programs investigated also some relatively transient members. Researchers had, in addition, a relatively stable support staff of technicians. Whereas these support staff do not directly feature in the analysis in this paper they still nonetheless shared some commitment to the goals of the group. It is important, thus, to distinguish between a "core" group of program members who were most influential over the events (as revealed from the interviews including those with non-core members, and from published research findings) and a larger group of technicians, research students and colleagues who were often as fully involved in the activities of the group, but who were not the major decision makers. The groups had (and still have) program leaders, but over the period under study there appeared to be a relatively balanced sharing of power amongst the three core members. That is to say, the authority of the group leaders did not appear to detract from the relative autonomy of the other two core members. With respect to the other program members, the structures of authority are taken ("until further notice", as it were) as typical of the situations of research student, technical assistants, professional officer, and colleague.

5.4 The method of repeated feedback used in the research

Once a program of research had been identified (by processes which will be discussed in Section 5.6) the general strategy used was as follows. Initially as many as possible scientists who had some involvement with the program were interviewed in a relatively open-ended fashion. This open-endedness was desirable in order to gain the subjects' points of view with a minimum imposition of my own prior expectations. These interviews formed a basis for continued interaction with the most centrally involved scientists. This interaction took the form of an in-depth analysis of their research. A questionnaire containing a reconstruction of the scientists' research goals, theoretical landscape, significant research events and social factors significant in the establishment of the goals was administered to the scientists as soon as the interview material and my own reading enabled me to gain a relatively comprehensive view of the research program (see Appendices 3 and 12). In both cases this occurred soon after interviewing began - most of the material used did in fact, emerge from the first round interviews. The scientists' reactions, corrections and comments about this material were gathered during a series of follow-up interviews. On the basis of this updated information a second round reconstruction and questionnaire was administered (see Appendices 4 and 13). Another series of follow-up interviews provided some additional material which has been incorporated in the two accounts. In both case studies a wide ranging consensus had in fact, been achieved after the second round questionnaires, so the accounts that will be presented are as complete as possible (with respect to the description of the two research programs - their evaluation in sociological terms has not, however, been the subject of major discussion with the scientists). The scientists were asked not to collaborate over their responses to the questionnaires: all agreed, and so the consensus that I have presented is hopefully as valid as possible given the method employed.

This method of repeated feedback can be represented diagramatically as shown in Figure 5.4-1.

FIGURE 5.4-1: The "method of repeated feedback"



Activity in the feedback loop can in principle, continue indefinitely, but clearly, at some point it may be necessary to use the sociologically mediated synthesis of information as an input to some other process (such as a thesis incorporating this synthesis). As implicit in the discussion, this moment of exit from the feedback loop is optimal once a consensus over a description of the research program has been reached between the sociologist and his respondents. Note that this consensus need not extend to all aspects of the sociologist's interpretations; nor indeed, is there meant to be an implication that description is fully separable from interpretation - a partial separation only is ever possible, but this separated account can, as the case studies demonstrate, provide a measure of relatively "neutral", and "objective" territory from which to proceed.

On the question of validity it should be clear from the description of the method employed that the iterative procedure that has been used has an in-built test of validity: given that one of the best tests of any reconstruction of events is to ask respondents whether a particular account meets up with their own recollections (particularly if there are a number of respondents), any method which depends on non-collaborative consensus of opinion about an account has at least some claim to validity.

But, of course, even in the event of total disagreement any particular research account would not be necessarily invalidated given that no individual can be expected to have more than a partial and perhaps distorted view of events. However, in the case of a wide ranging consensus between analyst and respondents (as was obtained in the case studies presented in this thesis) all one can reasonably do is stay on the alert for confounding data. It is important to appreciate that once a group of people have reached a consensus about events of which only they have most complete memory (because they were the protagonists), then that consensus will be relatively final (save respondents) breaking down and confessing to some deliberate lies, or save respondents breaking through to new memories and insights - neither of which event is likely after a period of sustained interaction over a period of one or two years).

This particular method of repeated feedback which has been used to generate much of the data in this thesis has, so far as I am aware, not been previously employed in the sociology of science (or for that matter elsewhere in sociology - although the general principles of the Delphi Technique upon which the method is based certainly are; see Section 5.5).

5.4-1 Other interviews conducted

The production of this thesis involved the use of other empirically based material. In addition to material available from the published and unpublished literature available to me, interviews were conducted with well known (and mostly "elite") researchers who were associated with the specialty areas of the research programs. The purpose of these interviews was, as discussed earlier, to gain "outside" commentary on the research programs and to enlarge my understanding of my respondents' beliefs about their own research programs. These interviews were also conducted in a relatively open-ended way, but were guided by my own, at that stage, fairly well formed ideas about the history and significance of the particular research programs being investigaged. Seven "outside" scientists were interviewed about solar energy research and five "outside" scientists were interviewed about neuro-pharmacology.

Many other scientists and "lay" people have also been interviewed and engaged in conversations during the production of this thesis.

These discussions have provided part of a general background for this thesis, but in one specific way they have contributed quite tangibly to particular findings. That is, the Solar Energy Belief System, which will be discussed further in the next section, was partially constructed on the basis of experience and information gained over many years. This basis derived from the period of my own training as a Chemical and Fuel Engineer and my efforts to learn more of energy production in the context of "alternative technology" and "alternative" life styles. This latter interest involved fieldwork in England and Australia, the most significant interactions having occurred in rural Suffolk and Wales, Manchester, Bath, and the "Rainbow Region" of the North Coast of Australia. In addition, during the period 1974-1977 I attended conferences and festivals devoted partially or wholly to considerations of energy in London, Manchester, Bath, Sydney, Canberra and Bredbo.

5.4-2 The generation of the Solar Energy Belief System

In the case study which follows a "Solar Energy Belief System" has been identified. Empirically speaking, this implies that solar energy researchers were sufficiently cognitively institutionalised to enable the identification of persistent patterns of beliefs which recurred in particular communicative contexts, irrespective of the particular type of solar energy research being conducted. That is, it was observed that these beliefs were thematically related, mutually reinforcing and relatively autonomous.

This process of identification involved interviewing, literature surveys and the synthesis of my own understanding of the field. In other words, the Solar Energy Belief System was drawn from a variety of sources. In the first instance it was constructed as a way of summarising the beliefs of researchers who were writing about solar energy. Such a table was in fact quite easy to draw up since the literature surveyed demonstrated a fairly obvious consensus on the points listed. The literature surveyed was however, largely restricted to "elite" solar energy researchers, that is, to scientists who had established a reputation and who appeared to function fairly regularly as spokes-people. Naturally, it was suspected that the views of such an apparently biased sample might not reflect the rank and file of researchers interested in solar energy. This fear was not substantiated - even after a period of three years since the initial formation of a structure of beliefs very similar to that presented in Table 6.6-1 [see Jagtenberg, 1975], the same beliefs still appear to be held across the elite and rank and file of solar energy researchers. As mentioned in Section 5.4-2, this substantiation is based on a program of formal and informal interviewing of Australian solar energy researchers, and also on the basis of conversations with all manner of scientists over a period of three years.

5.4-3 The theoretical basis for the method of repeated feedback

The method of repeated feedback described in the last section was particularly inspired by the emergence of a fairly widespread interest in processes of negotiation of meaning - most significantly the Delphi technique, which is often used in management consultancy (see for example, Stander and Rickards, 1975], the negotiations of meaning in any customer-client situation, but particularly in psychiatry [Scheff, 1968] and psycho-analytically inspired theorising about hermeneutics [eg. Radnitzky, 1968]. This iterative process of negotiation has also been developed in this thesis in the light of Mannheim's postulation of three levels of meaning [see Section 2.1]. That is, I have been concerned to develop a methodology that at least incorporates an awareness of the shifts in meaning involved when analyses of institutionalised processes are generated from individual accounts. I take this concern to be congruent with a more "interpretive" perspective in the sociology of science, that is to say, a perspective which does not lose sight of the importance of the individual "actor", and which is concerned with the interactions between individual and institution [cf. Law, 1974; Law and French, 1974; and Hill, 1979]. In other words, one of my main concerns has been to do a good ethnography in order to establish the nature of individual actor's cognitions [cf. Law, 1979, op.cit].

Considered more broadly, one of my basic interests has been to develop some of the insights available from a broad range of phenomenologically influenced theorising into a relatively accessible methodological approach. Thus, for example, whilst I am not overconfident of having reached the phenomenological heights of penetrating to the absolute essence of the phenomena presented here, nor of having distinguished the essential relations of the phenomena altogether intuitively, I have certainly attempted to investigate particular phenomena without definite pre-conceptions of their nature [see Bruyn, 1966, for further examples of typical phenomenological prescriptions].

5.5 The process of selection of the research programs to be investigated

Initially, a number of informal discussions were conducted with a broad range of scientists and engineers. The sample was largely selected from a network of contacts that had been established during the periods of my undergraduate training in chemical and fuel engineering (1970-1973) and my subsequent employment as a professional officer in the Physical Chemistry department of a large Australian university (1974). The major selection criterion employed here was that the research represented in the sample should cover a very wide range of interests in a wide range of disciplines. Only after I had sampled the diversity of research in the natural sciences could I possibly make decisions about the criteria for choosing particular programs for further investigations. I had already, at that time, some firsthand knowledge of what this diversity was, but my experiences were somewhat dated by 1977 and felt to be in need of augmenting. Informal interviews were conducted during February 1977 [see Appendix 2 for further details].

On the basis of these interviews a number of research programs were selected as possible subjects for further research. These programs covered a very broad spectrum and so some process of selection had to be devised. Three very loosely defined (at that stage) variables were used to sort the programs:

(a) Application orientation - where was the program situated on a continuum between basic research and strongly practice oriented research?

(b) Level of "maturity" (M) - how strongly institutionalised was the research? This was operationalised as a heuristically based estimation of the level of cognitive institutionalisation only.

(c) Number of disciplinary perspectives involved in the program (D).

These variables were chosen as the most discriminatory and empirically accessible dimensions of the concept of institutionalisation. The first two variables are identical with the two "major variables" defined in Section 5.2. Variable (c) is however, a sub-variable of the level of institutionalisation of research. That is, the process of selection that was employed in 1977 is consistent with the theoretical analysis developed in this thesis. On the basis of these variables the research programs sorted out as follows:

(i) Basic research/weakly practice oriented research:

		D	
		1	> 1
M	Low		ℓ-dopa*
	Low-medium		
	Medium	Turbulent flames	
	Medium-high	Anions; Detergents	
	High		Colloids

* Note: All the programs have been labelled according to their most general subject concerns.

(ii) Strongly practice oriented research:

		D	
		1	> 1
M	Low		
	Low-medium		Bagasse burning; Coal-oil conversion
	Medium		Air pollution
	Medium-high		Hydro-metallurgy Solar energy
	High		

Constraining factors based on considerations of time, money and methodological requirements prompted a decision that two research programs would be a sensible number to entertain as subjects for intensive research. One program would not enable comparisons (since, as it has been pointed out, the literature is not rich in suitable case studies) and the added returns on any more than two case studies would be questionable particularly given the pioneering nature of the exercise in the first place. To facilitate comparisons two programs that were as widely separated as possible (with respect to levels of application orientation, maturity and number of disciplinary perspectives involved) were selected. There were other considerations that contributed to the final decision, however. Those programs where I was particularly welcome as a sociologist were naturally more inviting than those where the reaction to my presence was less of a known quantity. Those programs that were conveniently geographically located were also more inviting than widely dispersed programs. When all factors were taken into consideration the final choice was two university based programs (but different universities): one being an investigation of the neurologically active substance ℓ-dopa and its metabolites, and the other being an investigation of selective surfaces and their use in economically feasible solar heating systems. For the sake of convenience I have labelled these programs the Dopamine/ Octopamine Program (DOP) and the Selective Surfaces Program (SSP). Inspection of the above diagrams shows that these two programs were considered to be widely separated along the dimension of "maturity", or level of cognitive institutionalisation, and level of practice orientation. Both programs were multidisciplinary however, and so this variable became of less interest in subsequent analysis (that is to say, if a larger sample had been possible it would have been possible to compare a single discipline based program with multi-disciplinary programs). The two programs were conducted as part of the research activities of a clinical pharmacology department and a physics department (in different universities). The restriction of my research to a university environment is perhaps a limitation that has been shared by too many previous research programs in the sociology of science, but it was anticipated that the solar energy program would be sufficiently practice oriented to overcome the institutionally derived "pure research" bias that restricts the generality of much work in the sociology of science.

5.6 Specific methodological details concerning the two case studies

(a) The DOP. The empirical material used in the case study was largely gathered over a period of three years starting in January 1976. Since research on the DOP was actually commenced before research on the SSP, the experiences gained were of value in organising field work on the SSP. That is, the DOP study functioned initially as a methodological pilot, but this situation did not last much longer than six months by which time research on the SSP had begun and fairly quickly reached a parallel level of development. Nonetheless, more time was spent in an informal way with the DOP researchers and the initial questionnaires were subject to a longer period of deliberation. This was countered, however, by a greater confidence with the initial approach to be employed with the researchers in the SSP.

Unstructured interviews were conducted with all five of the active dopamine/octopamine program members. The sixth member who was on leave over the period of intensive interaction was not interviewed. Six other members of the department were interviewed and some interaction and conversation was obtained with all seventeen members of the department. Most of the initial interviews were conducted during seven visits to the laboratory over a period of one month, but additional discussions with department members occurred over a period of three years. Most of the visits to the laboratory occupied a whole day and consequently it was possible to participate in a range of department activities, the most frequent of these being morning and afternoon teas and lunches. Other activities in which I was a "participant-observer" included a student-staff seminar, the annual inspection, a clinical "round" and a farewell party for one of the scientists. In depth interviews were initially commenced with three of the program members but after twelve months one of these "core" members left for overseas. Contact was maintained but unfortunately the intensity was diminished by the distance. Nonetheless, the contact that was gained through the early stages of my field work was a vital contribution to the subsequent form of the results.

The first and second round questionnaires which formed a basis for continued interaction are contained in Appendices 12 and 13.

(b) The SSP. The empirical material used in the case study was largely gathered over a period of two and a half years starting in July 1976. Initially eight scientists who were closely involved with the then recently established research program were interviewed. These interviews formed a partial basis for continued in depth interviews with five of the most centrally involved "core" scientists.

A reconstruction of the evolution of the Selective Surfaces Program based on transcripts of interviews with program members has been presented in Appendix 6. This reconstruction has been broken down into the following sections:

1. The establishment of the solar energy program;
2. The evolution of the program
    (a) Technical decisions,
    (b) Mediating influences;
3. The Future.

A similar reconstruction has not been presented for the DOP since such a document would not significantly add to the analysis contained in this thesis. The main point of including this particular reconstruction of the SSP is to demonstrate the depth of the information available from the interviews and the kind of processes that were involved in the generation of the two research accounts. The first and second round questionnaires for the SSP are contained in Appendices 3 and 4.

All the interviews conducted were tape-recorded and transcripts of the most relevant sections of the interviews were prepared. The bulk of the transcripts have not been included in the Appendices for reasons of confidentiality and excessive volume. Moreover, apart from their inherent ethnographical interest, the transcripts are often of limited relevance due to the diffuseness which tends to result from open-ended techniques.

5.7 Summary of the organisation of the fieldwork and the techniques used

On the basis of the material that has been presented so far in this chapter it is possible to summarise the field work as a sequence of events involving particular techniques.

1. Two groups of researchers were selected as subjects.
2. Exploratory discussions with the group leaders and program members were held.
3. The research of the two groups was investigated through my reading of some of the groups' publications, other scientists' publications, general texts and discussions with a variety of

sources.

4. Open-ended interviews were conducted with all the group members.
5. In depth work with selected group members was pursued using, in chronological order
   i. open-ended interviews;
   ii. a questionnaire including a preliminary description of the research program - this questionnaire was constructed largely on the basis of the interview material that had been gathered so far;
   iii. discussion of questionnaire responses;
   iv. updating of analyses on the basis of the previous discussions;
   v. feedback of "first round" synthesis of questionnaire responses and discussion of responses in the form of a "second round" questionnaire, and updated description of the research program;
   vi. discussion of second round questionnaire responses;
   vii. updating of analysis;
   viii. follow-up discussions.
6. Open-ended interviews were conducted with elite members of the specialty group.
7. The material gained from the interview processes was written up in the form of paper which provided a critical perspective.
8. These reports were circulated amongst program members.

5.8 Indicators used in the description of the empirical fields and in the investigation of the hypotheses

Although this thesis is an exploratory study and did not evolve in a totally pre-planned fashion the field work was pursued with a number of hypotheses in mind. These hypotheses which were summarised in Section 4.9-1 were operationalised in terms of two major variables and a number of different indicators. These hypotheses involve one of, or both, the following major variables:

1. The level of institutionalisation of research;
2. The level of application orientation of research.

Analytically it is possible, as we have seen in Section 2.8, to distinguish between cognitive and social institutionalisation - we could conceivably then speak of these two aspects as sub-variables. This separation is not really necessary however, and as shown in Table 5.8-1 there are useful indicators which have both a cognitive and social aspect. Consequently, therefore, only such sub-variables as actually identified in the hypotheses have been separated. These will be listed with the details of hypotheses and indicators in the next section. These major variables have a number of different indicators, as shown in Tables 5.8-1 and 5.8-2 which follow.

TABLE 5.8-1: Major indicators of the level of institutionalisation of research.

Orientation of indicators of levels of institutionalisation*			Type/function of sub-variables (after van den Daele and Weingart, 1975)
	Cognitive	Social	
1.	Comparisons of aspects of reconstructions of the evolution of significant research events in the two programs		Theoretical; Methodological/technical
	(a) Degree of co-ordination of labour		
	Aspects of:		
	(b) goal formation		
	(c) goal evolution		
	(d) goal achievement		
	(e) Degree of serendipity in research		
	(f) Node analysis of flow diagrams of the research process		
2.	Cognitive structure comparisons		Scientific relations
	(a) Dimensional comparisons		
	(b) Professional orientation of cognitive structures		
	(c) Stability of cognitive structures		
	(d) Degree of articulation of belief systems		

* Where the first word of entries has been listed directly under the column headings "cognitive" or "socially" they are conceived as being so oriented. Where the first word of the entries has been listed between the two headings they are conceived as having a "mixed" nature.

TABLE 5.8-1 (cont.)

Orientation of indicators of levels of institutionalisation			Type/function of sub-variables (after van den Daele and Weingart, 1975)
	Cognitive	Social	
3.	Autonomy comparisons:		Differentiation; Integration; Reproduction; Theoretical
	(a)	Autonomy index	
4.	"Marginality" comparisons:		
	(a) Academic status		
	(b)	Level of funding	Integration; Differentiation; Reproduction
	(c)	Security of funding	
	(d)	Security of staffing	
	(e)	Integration with science based industries	

TABLE 5.8-2: Indicators of level of application orientation of research (along a continuum between basic, "curiosity oriented" research and practice oriented research)

5. Professional orientation of cognitive field (restricted to analyses of goals and theoretical landscapes).

6. Goal orientation of publications.

7. Scientists' self evaluations.

8. Assessment of the contexts of research and legitimation.

5.9 Hypotheses explored and the general nature of the variables which will be used in their exploration

In Table 5.9-1 the hypotheses listed in Section 4.9-1 have been listed in conjunction with the broad category of the variables which will be used in their exploration in the case studies which follow. These broad categories have been drawn from Table 5.8-1. Although particular variables have been selected as more relevant to particular hypotheses it must be stressed that this selection is somewhat arbitrary - all the variables provide an overall matrix, some components of which will emerge more prominantly than others as interpretationally relevant depending on the particular themes and goals in the sociological analyses which follow. The following Table is intended, therefore, as a heuristically useful generalisation and not as an analytical straitjacket. The variable pattern outlined below will be used however, as the basis for more quantified comparative analysis which will follow the two case studies.

TABLE 5.9-1: Hypotheses explored as the broad category of relevant variables.

Hypothesis	Variable (numbers as listed in Tables 5.8-1 and 5.8-2)																			
	1						2				3	4					5	6	7	8
	a	b	c	d	e	f	a	b	c	d	a	a	b	c	d	e				
H1. Scientists are subject to the social and cognitive control of professionalism which operates through the agency of professional orientational reference groups								x				x	x	x	x	x	x		x	x
H2. Professional orientational reference groups provide a basis for scientists' distinctions between, and definitions of, scientific and non-scientific activity		x	x	x				x		x		x	x	x	x	x	x	x	x	x
H3. Scientists tend to bracket social considerations as "external" to the research process			x	x					x	x								x	x	x
H4. Scientists move in thought and action between two sub-universes of meaning: a context of research and a context of legitimation			x	x					x	x								x	x	x
H5. This movement between sub-universes may engender in scientists a conflict of relevancies			x	x				x				x	x	x	x	x			x	x

TABLE 5.9-1 (cont.)

Hypothesis	Variable (numbers as listed in Tables 5.8-1 and 5.9-2)																			
	1						2				3	4					5	6	7	8
	a	b	c	d	e	f	a	b	c	d	a	a	b	c	d	e				
H6. Scientific research occurs in the context of a structured cognitive field which consists of interpenetrating levels: metaphysical, theoretical, subject concern, and technical levels							x	x	x	x	x						x	x	x	x
H7. Cognitive structures provide structures of relevance for research	x	x	x	x	x			x	x	x	x	x	x	x	x	x		x		
H13. Scientific research varies in its orientation towards social application		x	x	x	x			x		x								x	x	x
H14. Practice oriented research is more highly constrained by social, economic and political factors than is basic research		x	x	x	x					x	x	x	x	x	x	x		x	x	x
H8. Scientists are directed in their research towards a wide range of goals which span different levels of the cognitive field of a research program			x	x			x	x										x		
H9. Not all the goals that are perceived by scientists to be relevant to their research remain equally relevant			x	x							x							x		

TABLE 5.9-1 (cont.)

Hypothesis	Variable (numbers as listed in Tables 5.8-1 and 5.9-2)																			
	1						2				3	4					5	6	7	8
	a	b	c	d	e	f	a	b	c	d	a	a	b	c	d	e				
H10. The goals of scientists change over time									x											
H11. Scientific research is predominantly instrumental by virtue of being more highly directed towards technical goals and the means for their realisation than towards the value of these goals			x	x						x	x							x	x	
H12. Most scientists perform research as part of a research program which is constituted through the collective activities of a group of research workers who share a commitment to particular research practices, who are directed in their research towards a shared set of goals, and who share, to some extent, a common stock of specialised knowledge	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
H15. The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated to the level of scientific marginality of the program										x		x	x	x	x	x			x	x

TABLE 5.9-1 (cont.)

Hypothesis	Variable (numbers as listed in Tables 5.8-1 and 5.8-2)																			
	1						2				3	4					5	6	7	8
	a	b	c	d	e	f	a	b	c	d	a	a	b	c	d	e				
H16. The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated to the level of orientation of program members towards more technical goals			x	x						x								x	x	x
H17. The level of institutionalisation of the context of legitimation of a scientific research program is negatively correlated to the level of orientation of program members towards higher level goals				x														x	x	x
H18. The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated to the level of practice orientation of research in the program										x									x	x

5.10 Summary of the overall design of the thesis

There are two different levels of organisation contained in this thesis. The first level is the actual design of the research which has preceded the writing of this thesis. This theoretical, methodological and empirical activity has been in effect, incorporated into the theoretical framework and empirical data which now constitutes this thesis. So far we have then discussed at some length the historical dimension of this thesis. By way of conclusion of this methodology section and by way of orientation towards the empirically based material which is to follow, the second level of organisation of this thesis will be reviewed in summary form. This level concerns the logic of development of the thesis as it has been presented and as it will be presented in the remaining chapters.

1. The assessment of the "state of the art" of sociology and other studies of science.
2. The development of theory on the basis of criticisms of the literature and my own particular theoretical interests.
3. The development of some general hypotheses.
4. The exploration, development and testing of hypotheses:
    i. Choice of indicators of key variables;
    ii. Descriptions of empirical fields in case studies;
    iii. Testing and development of significant hypotheses;
    iv. Comparison of different case studies;
    v. Testing and development of particular significant hypotheses;
    vi. Overall synthesis and general conclusions.

This summary contains the overall *logic* of development by this thesis. Be that as it may, the material contained in the chapters which follow will not be presented in such a strict order - for the sake of clarity, and the most persuasive development of the analysis. Nor indeed, does the "plan" fully reflect the *actual* path of development of the thesis, which was much more dialectic than can possibly be indicated in a sequential list. Actual events were highly inter-related, all four stages having constituted several progressively developing cycles of evolution of analysis; the major criterion for evolution from one cycle to the next was the perception of possibility of an analysis of increased validity and generality.

CHAPTER 6: INTRODUCTION TO THE CASE STUDIES AND CASE STUDY 1: ASPECTS OF THE INSTITUTIONALISATION OF AN AUSTRALIAN SOLAR ENERGY RESEARCH PROGRAM

6.1 The purpose and scope of the case studies presented in this thesis

This case study is the first of two similar studies - the second study which follows concerns an Australian neuropharmacological research program. For logical reasons I have chosen to use this first case study as the major explanatory vehicle. The second case study, whilst being a self contained study of approximately equal empirical depth has been used more as a point of comparison than exposition. In both the case studies I have operationalised the broad concepts of cognitive and social institutionalisation, cognitive field, cognitive structure, professionalism, and research program. In attempting that I have used many of the phenomenologically derived concepts that were introduced in Chapters 2 and 4 - for example, the concepts of finite province of meaning, horizons of meaning, projects of action as goal directed, and types of relevancy. In addition to these basic concepts, I have attempted to capture the fragmented mode of the institutionalisation of modern science through the concepts of "context of research", "context of legitimation" and "double bind", as developed in Chapter 4.

Both the case studies have been largely organised to elucidate, assess and develop the hypotheses that were introduced in Chapters 4 and 5. These hypotheses have been explored using many of the variables listed in Table 5.2-3 as relevant to the hypotheses. Not all of the variables listed in that table have been used with equal emphasis, however - that is, not all of those variables have proved to be equally necessary or useful in the analyses which follow. A more systematic and comparative assessment of these variables will follow the case studies in a more quantitatively comparative section. Nonetheless, even though some of the "relevant" variables listed in Table 5.2-3 have not been explicitly discussed in reference to particular hypotheses, this does not mean that they have been irrelevant to the analyses which follow. As mentioned in Section 5.2-1 *all* of the variables listed provide an overall matrix, some components of which emerge more prominantly than others as interpretationally relevant, depending on the particular themes and goals in the sociological analyses which follow.

In summary, the main purpose of this chapter has been to elucidate the mode of institutionalisation of a particular research program. This will entail the determination of the various structures that define the context of the research program and their relationship to processes of research on the research program.

6.2 A summary comparison of the two case studies

The two research programs which are investigated in the case studies which follow have a number of significant similarities and differences. Their institutional setting and size of the programs are the most obvious points of similarity - both are university based programs of a small to medium size of eight and five scientific members. Both programs were also interdisciplinary in nature, scientifically "marginal" in various respects, and relatively new at the commencement of my research. It is the difference between the two programs that will however, provide a basis for comparison. The programs had different disciplinary bases, different levels of practice orientation, different levels of institutionalisation (including different levels of funding) and different legitimation needs. These differences have been explored through the hypotheses which were developed on the basis of the theoretical structure of the thesis.

On the basis of these hypotheses which were listed in Section 4.9-1, Table 6.2-1 contains a summary comparison of the two case studies which follow. This table indicates both the overall type of support and the range of the support provided by the data. As shown this support has been classified as either positive, negative or indeterminate. The negative data has discussed at some length in Chapters 6 and 7, but the full impact of this negative impact is most clearly registered in Chapter 8, where some of the hypotheses have been revised in the light of that data. All these changes have however, been foreshadowed in Chapters 6 and 7. A more detailed comparison shown in Table 6.2-1 will follow in Chapters 7 and 8. The Table has been presented at this early stage mainly to provide an overview of the material which follows. In this overview it is important to note that at a very general level the evidence demonstrates that the levels of cognitive and social institutionalisation of the DOP are lower than those of the SSP.

Some of the Table entries have been bracketed together to give an overall tendency. This is meant to indicate something of the range of support for particular hypotheses that may be gained from a consideration of different areas of the data. In other words, the hypotheses have been evaluated in the light of a range of data, some of which was inconsistent with respect to particular hypotheses.

TABLE 6.2-1: Summary comparison of the two case studies

Hypothesis	Type of support contained in the case studies - positive (+), negative (-), or indeterminate ($\not+$)		
		Range of Support	
	Overall	Case Study 1: SSP	Case Study 2: DOP
General Hypotheses:			
H1. Scientists are subject to the social and cognitive control of professionalism which operates through the agency of professional orientational reference groups	+	+	+
H2. Professional orientational reference groups provide a basis for scientists' distinctions between, and definitions of scientific and non-scientific activity	+	+	+
H3. Scientists tend to bracket social considerations about their research as "external" to the research process	+	$\overbrace{+, -, \not+}^{+}$	$\overbrace{+, \not+}^{+}$
H4. Scientists move in thought and action between two sub-universes of meaning, a context of research and a context of legitimation	+	$\overbrace{+, -, \not+}^{+}$	$\overbrace{+, \not+}^{+}$
H5. This movement between sub-universes may engender in scientists a conflict of relevancies	+	+	$\overbrace{+, -, \not+}^{\not+}$
H6. Scientific research occurs in the context of a structured cognitive field which consists of interpenetrating levels: metaphysical, theoretical, subject concern, and technical levels	+	+	$\overbrace{+, \not+}^{+}$

TABLE 6.2-1 (cont.)

Hypothesis	Type of support contained in the case studies - positive (+), negative (-), or indeterminate ($\not+$)		
	Overall	Range of Support	
		Case Study 1: SSP	Case Study 2: DOP
General Hypotheses (cont.)			
H7. Cognitive structures provide structures of relevance for scientists' research	+	$+, \overset{+}{\frown} \not+$	$+, \overset{+}{\frown} \not+$
H8. Scientists are directed in their research towards a wide range of goals which span different levels of the cognitive field of a research program	+	+	+
H9. Not all the goals that are perceived by scientists to be relevant to their research remain equally relevant	+	+	+
H10. The research goals of scientists change over time	-	-	-
H11. Scientific research is predominantly instrumental by virtue of being more highly directed towares technical goals and the means for their realisation than towards questions of the value of these goals	+	$+, \overset{+}{\frown} -, \frown \not+$	$+, \overset{+}{\frown} -, \frown \not+$
H12. Most scientists perform research as part of a research program which is constituted through the collective activities of a group of research workers who share a commitment to particular research practics, who are directed in their research towards a shared set of goals, and who share, to some extent, a common stock of knowledge	+	$+, \overset{+}{\frown} \not+$	+

TABLE 6.2-1 (cont.)

Hypothesis	Type of support contained in the case studies - positive (+), negative (-), or indeterminate (≠)		
	Overall	Range of Support	
		Case Study 1: SSP	Case Study 2: DOP
General Hypotheses (cont.)			
H13. Scientific research varies in its orientation towards social application	+	+	+
H14. Practice oriented research is more highly constrained by social, economic and political factors than is basic research	+		
Hypotheses specifically oriented towards comparing the two programs *			
H15. The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated to the level of scientific marginality of the program	+		
H16. The level of institutionalisation of the context of research of a scientific research program is positively correlated to the level of orientation of program members towards more technical goals	+		

* These hypotheses have not been systematically dealt with in the case studies. They have however, been evaluated in Chapter 8.

TABLE 6.2-1 (cont.)

Hypothesis	Type of support contained in the case studies - positive (+), negative (-), or indeterminate (≠)		
		Range of Support	
	Overall	Case Study 1: SSP	Case Study 2: DOP
Hypotheses specifically oriented towards comparing the two programs (cont.)			
H17. The level of institutionalisation of the context of legitimation of a scientific research program is negatively correlated to the level of orientation of program members towards higher level goals	-		
H18. The level of institutionalisation of the context of legitimation of a research program is positively correlated with the level of practice orientation of research in the program	+		

6.3 Introduction to the Solar Energy case study

In a decade marked by an increasing global "energy consciousness" it is sobering to reflect on the likely sources of change in the traditional mode of non-renewable resource based energy production. For although radical changes such as the increasing utilisation of solar energy generating systems are highly likely, one important source of new ideas and techniques will continue to be a highly institutionalised and traditional mode of scientific production.

Further, although solar energy research has in the past been supported and occasionally stimulated by more "counter-cultural" interests in self-sufficiency, ecology and "alternative technology", the youthful enthusiasm of the Fifties and Sixties has mostly faded into fond memories, and solar energy research is increasingly having to deal with the reality principle of the market place. Nonetheless, solar energy continues to remain as a topic of some urgency amongst many energy conscious people whose fears for the future continue to be fuelled by a rising suspicion of nuclear power stations and the "friendly atom". In fact, the whole subject has gained something of a cult flavour, solar energy often being promoted as a kind of perfect master amongst the various energy options.

However, the brute facts of finite fossil fuel resources, energy crises, and an increasing public awareness of the hazards of the nuclear industry continue to provide solar energy enthusiasts with powerful arguments. Indeed, it seems hard to deny that changes in our mode of energy production will necessarily stem from these "brute facts". But, as history testifies, change is mostly born of arduous struggle and even then, promising beginnings often wax and wane with the phases of social change. The fortunes of solar energy research are no exception to this rule and thus we may still live to witness the blossoming of a solar age, but who knows after what kind of preceding energy disasters.

Change presupposes order however, and when one deals with the physical sciences one is dealing with a highly ordered phenomenon. Ironically then, those solar energy researchers contributing to changes in our understanding of the possible energy futures of the globe are usually operating within highly ordered sub-universes of meaning in which there is often an enormous resistance to changing ideas and practices. In such a rigid environment scientists may only be the partially willing and partially conscious agents of broader changes.

This rigidity of structures which still continues to make an encounter between scientist and lay person an often strained affair, particularly if cherished beliefs are being challenged, is compounded by a tendency of scientists to bracket the social aspects of their research as "external" to research. In the case study which follows activity as members of a research program tended to be divided between two contexts - a context of research and a context of legitimation, both of which were highly institutionalised. This division within the sub-universe of the research program is understood to be a mechanism by which a conflict situation is made more bearable [see Section 4.7]. One of the primary conflicts that is dealt with in the case studies concerns the demands that scientists experience to be both professionally competent and socially useful - two demands which tend to be associated with different criteria of evaluation.

The case study presented in this chapter is then broadly concerned with aspects of the cognitive and social institutionalisation of a particular group of Australian physicists. Two of the goals that were important in directing the research of these physicists and which conveniently summarise much of the work on the program were firstly, to develop a new and efficient selective surface (which is basically a means of improving the efficiency of solar collection of a surface), and secondly, to develop a commercially viable collector which will employ the new surface. The first goal has been achieved, but the second goal remains as a major direction for research and development - although a promising solar collector has been demonstrated, the commercial viability of the device cannot be known until more work, particularly on the development side, has been completed.

6.4 The institutionalisation of solar energy research in Australia: a brief overview

Summary: *In general, solar energy research in Australia is not highly institutionalised and exists as a relatively marginal group of loosely related research practices.*

Solar energy research in Australia is a relatively marginal group of loosely related research practices. The actual range of research activities that could be described as constituting a solar energy related "field" is in fact quite large: Table 6.4-1 shows the three broad areas of interests (by type of energy conversion concentrated on) and lists the major disciplinary and organisations that were engaged in the various types of research in 1976. In can be seen that the field outlined spans a number of disciplines: physics, chemistry, biochemistry, chemical engineering and mechanical engineering, to name the major divisions. Research programs in the field are often interdisciplinary in nature - this being a consequence of the field's tendency towards the solution of practically oriented problems, a tendency which usually strains disciplines with a fundamentally theoretical bias (such as physics). [1] For example, the particular research program discussed in this paper is described as being oriented to both physics and mechanical engineering (these being the two major professional orientational reference groups for the scientists involved). Most of the organisations in solar energy research in 1976 were restricted to the CSIRO and the universities. I have found no evidence of any significant research involvement in the private sector to 1976, a situation which is consistent with the general "marginality" of the field. [2] The list of organisations presented in the Table would need to be supplemented to bring it up to date, but it is presented as a summary of the institutionalisation of solar energy research in Australia at the time when the particular solar energy program being considered in this paper was well established.

Most of the research covered by Table 6.4-1 is "marginal" by the following indicators:

(i) Considered alongside other areas of energy research, solar energy research is not heavily funded. Accurate figures are not available, but in 1976 the $1.5 million (approximately) [3] that was spent in Australia is less than half of one percent of the total national R & D budget. Despite the lack of accurate figures one can be quite confident of the fact that the level of funding of solar energy research in Australia is on an approximate par with all the other advanced western nations: generally minute compared with other areas in the energy R & D budget. [4]

(ii) Many of the research areas that constitute the broad field of research are not held in high academic esteem by either solar researchers or their colleagues in other fields. This is

TABLE 6.4-1: A broad summary of the institutionalisation of solar energy research in Australia.+

Energy conversion type	Major disciplinary bases	Organisations strongly involved in related research in 1976*
Photochemical (e.g. photo-electrical cells)	Biochemistry Chemical Engineering Chemistry	Sydney University (SU) Australian National University University of New South Wales (UNSW) Australian Atomic Energy Commission Flinders University (FU)
Photo-thermal (e.g. flat plate collectors)	Mechanical Engineering Physics	SU; CSIRO (Divisions of Mechanical Engineering and Mineral Chemistry) South Australian Institute of Technology; FU
Photo-electric (e.g. silicon cells)	Physics	UNSW; AWA (UNSW)

* The Australian Physicist, 13, 11, 171-190 (November 1976). See also CSIRO, Solar Energy Studies, Report 74/1 for an account of Australian research at July 1974.

\+ Excluding most photobiological research, building research, and areas such as wave and wind power research which are related to the "indirect" utilisation of the sun).

particularly true of the main area of activity in Australia, photo-thermal conversion. This low academic respectability derives from a widespread feeling that the *theoretical* significance of the practice oriented problems that dominate the field is not high enough to form the basis for a professional career in science. That is, from the standpoint of competing specialty areas, solar energy research appears, generally speaking, to offer very few important "frontiers" type, theoretical problems. [5]

(iii) Recruitment procedures for solar energy specialists are largely ad hoc and often tied to small and unreliable research budgets. [6] This is an important consequence of the academic marginality discussed above. In Australia solar related research is thus not a popular Ph.D area. By and large, solar energy specialists have come to the field only after having established a career in a different area of research. [7]

(iv) The established mode of energy production is almost exclusively based on the consumption of non-renewable resources such as coal, oil, gas, and uranium. With the exception of certain specialist applications, solar based systems of energy production and solar related research programs are competitors with an established mode of energy production. [8] Since this mode is almost exclusively based on the consumption of non-renewable resources such as coal, oil, natural gas and uranium, any successful solar energy research program that was devoted at some level to actually gaining an increased utilisation of the sun as a source of energy entails both further challenging existing energy habits, and the inevitable political and economic struggles associated with any reorganisation of market patterns.

(v) Following from the last point, solar energy related research is then a competitor with other areas of energy research which have the support of, and are integrated with, an established mode of energy production.

These indicators are obviously connected in various ways; for example, an area of research that competes with a virtual monopoly will probably have difficulty gaining funding which in turn will limit research opportunities and restrict the flow of graduate students into the field. Furthermore, given that we are creatures of habit, the validity of anything that challenges prevailing theory and practices will always be regarded with suspicion, particularly so if this theory and practice is deeply enmeshed in an established social order. [9]

So far I have sketched a picture of solar energy research as not being highly institutionalised. Yet whilst it does not seem possible to speak of solar energy research as being anything as coherent as a "specialty" because the orientations within the areas of interest listed in Table 6.4-1 are fairly disparate in the way they are embraced by a wide range of established research fields, one is confronted with research practices that are not merely structured by organisational and technological exigencies and nominally linked together by the umbrella term "solar energy". There are definite indicators that some kind of community of interest does exist amongst solar energy researchers: there is an international society with its own journal [10] and an established network of national and regional branches, large scale international conferences occur with some regularity, and there are in existence innumerable smaller organisations, discussion groups, committees, sub-committees, etc., which are in various ways arguing for the neglected viability of solar energy as an energy resource. There do exist then commitments that are held in common amongst solar energy researchers, but in general the strongest consensus appears to occur more at an ideological level than at the level of specific research practices. Nonetheless, the field of solar energy research is institutionalised to some extent and in addition to the structures outlined in Table 1 it is possible to identify particular research areas where, as I have argued elsewhere, [11] the over-arching high level goal of "making the sun work for mankind by extracting large amounts of useful energy from solar radiation" has been translated into other general research areas apart from solar energy conversion; that is, solar energy collection and the storage, and transmission or transportation of energy derived from the sun. Socio-economic interests have further refined these areas into range of highly practice oriented research areas: thermal energy for buildings, provision of renewable non-polluting fuel sources, and electric power generation. Within these broad structures there are many different specific research goals which have been formed as a complex product of social, economic, theoretical and technical interests and demands. In the case study presented in this chapter, for example, two such goals have been to develop a new and efficient selective surface (which is basically a means of improving the efficiency of solar collection of a surface), and secondly, to develop a commercially viable solar collector which employs the new surface. Even goals at this level of specificity are shared by a number of researchers working in different research programs, but there is of course a limit to the sharing of research goals. At present in Australia, once the particular surface and type of collector have been specified, we find ourselves restricted, so far as I am aware, to individually unique research program.

Nonetheless, what is generally shared amongst solar energy researchers is a belief in the value of solar energy related research whatever its particular form. This belief has become coherently articulated over the years and it is now possible to identify a structure of related "sub-beliefs". We will return to this belief system in subsequent sections of this chapter. Overall, the nature of the existing solidarity is still however, too scattered to be called a "specialty"; perhaps we are witnessing a temporary alliance of researchers united by a shared sense of marginality in the face of the "obvious" social and economic importance of developing a system of energy production at least partially based on a resource renewable solar economy.

The fact that solar energy research is only a loosely related set of research practices (despite a shared ideological commitment) is reflected in the relative isolation of the selective surfaces program from other Australian solar energy research programs. There were only five Australian scientists outside of the local institution who were rated as important colleagues so far as the research was concerned.[12] In rough terms, this means that the selective surfaces program was conducted in relative isolation from seven of the eleven organisations listed in Table 6.4-1 as having solar energy research efforts.

6.5 The struggle for survival: a double bind situation

Summary: Researchers on the SSP were subject to the social and cognitive control of professionalism which operated through the agency of the professional orientational reference groups of science and engineering (H1). These reference groups provided a basis for the scientists' distinctions between and definitions of scientific and non-scientific activity (H2).*

The scientists appeared to operate in, and move between, two different contexts within the sub-universe of the research program - a context of research and a context of legitimation (H4). This movement between contexts (themselves sub-universes) was associated with a conflict of relevancies. This conflict was observed as a "double bind" situation where scientists experienced the, at times, conflicting demands to be both professionally competent and socially useful (H5).

We have seen some of the conflicts which define the marginality of solar energy research in Australia. In this section I shall continue to focus on the conflict situation which affected the scientists in this study, but an important correlate of this conflict situation will be introduced - the institutionalised separation of two contexts of relevance within the SSP. The solar energy researchers in this study found themselves in a "double bind" situation. As discussed in Chapter 4, rather more than simple conflict is implicit in this term, since some kind of socially problematic resolution to a conflict

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* All the section summaries which follow have been expressed, where possible, in terms relevant to the major hypotheses being evaluated in the thesis - that is, H1-H18 as listed in Section 4.9-1.

situation is also implicit: in fact, there are strong parallels between the situation experienced by the scientists discussed in this chapter and the "double bind" situation in which Gregory Bateson observed some children to find themselves [see Section 4.7]. That is, scientists in the study were subject to conflicting demands that were associated with negative sanctions, the scientists were emotionally committed to remaining in their situation, and there was in existence a socially problematic mode of dealing with the meanings of their research. In this last remark I am referring to the institutionalised separation of a "context of research" from a "context of legitimation" which was also discussed in Chapter 4. As mentioned there, this separation of meaning structures is probably a feature of most scientific research, but it is nonetheless problematic for being widespread. The problematic nature of this separation rests essentially in the way that through this mechanism questions of value are able to be bracketed from a domain of research which is thus able to remain primarily technically concerned. Thus, in all of the interviews that I conducted during both the case studies scientists consistently tended to associate "research" with technically oriented issues. The only way I could generally provoke social, economic and politically oriented commentary was to effectively redirect the dialogue towards the justification of their particular research, or solar energy research in general. In all the interview material it is clearly implicit that so far as the scientists and their colleagues were concerned their professional competence as scientists and their social relevance or value were two distinctly separate issues.

The institutional origins of the conflict situation being described here is understood to be largely in consequence of the way that research was conducted under the influence of a variety of different reference groups: the research program itself, scientific and engineering professional orientational reference groups, a solar energy "elite", various funding agencies, pressure groups, media groups, and so on. The fact that these groups provided a context of divergent and conflicting demands and interpretations meant that the generation of non-contradictory structures of motivational, thematic and interpretational relevance was problematic for the individual scientist. In conjunction with these conflicts (which we shall explore further in a moment) the institutional separation of a "context of research" from a "context of legitimation" appeared to be institutionalised. That is a *consistent pattern* emerged from the interview material - as described above the issue of professional competence was separated from issues of social relevance or value. In this pattern structures of relevance and their attendent stocks of knowledge which were actively utilised in research defined the field of research for the scientists. This field, or "context" was clearly distinguished from (that is, rarely discussed in close conjunction with) the values and beliefs which justified particular lines of research or made them particularly meaningful to researchers. This latter "field" emerged in a context of legitimation of research to "outsiders" who did not share the same commitments - for example, in the case of the solar energy researchers, through the group's press releases or individual member's discussions about the viability of solar energy. These two contexts were quite clearly experienced as different sub-universes of meaning with different role requirements and meaning compatibility of individual experience (as partially shared by me). These two contexts have been described at some length in this chapter in terms of shared structures of goals, knowledge and values, and the existence of distorted communication as information flows out of the context of research subject to legitimation needs.

What were the conflicts that most affected the researchers? In conjunction with the conflicts which defined the marginality of the specialty there was more immediate "professional" conflict so familiar to many academic researchers. In the case of physicists, survival as a professional physicist in a university requires the production of "good physics", of a kind acceptable by reputable and largely theoretically oriented journals, and on the other hand, survival as a professional physicist depends on the continued existence of a suitable job with research opportunities. This latter condition obviously depends on available funding, and in a climate of shrinking opportunities such as has been experienced by many Australian researchers over the last three years, this entails entrepreneurial skills and a "marketable" product. Generally speaking, the best kind of "marketable" product is obviously one that satisfies funders and professional colleagues alike. However, in a climate where the criteria of relevance associated with one's sources of funds are often not the same as those of physicist colleagues, a related conflict may arise. In the present case this conflict takes the form of the double bind situation of producing "good" physics by traditional academic standards, and also producing economically useful physics (by the standards of the business world). It hardly needs remarking that this conflict is not restricted to physics, nor is it unsurmountable: as discussed in Chapters 2 and 4, an increasingly typical feature of academic life in western universities is the balancing of largely "external" criteria of economic and social utility with traditional academic criteria of relevance. The point is that these criteria are still largely separated in the minds of academics and non-academics alike. The conflict is to some extent, "resolvable" if the university researcher so caught can manage to satisfy all the competing demands at once: for example, produce "good" physics, produce "useful" physics, and teach physics. The satisfaction of all these demands may require different physics for different situations, or if one is lucky (or cunning) the same physics for most situations. But, however the conflict is resolved (or not resolved) one thing remains, and that is the separation of "internal" academic criteria from "external" social and economic criteria, for no matter how the dichotomy is smoothed over there are two fundamentally different orientations and attitudes involved.

This conflict of structures of relevance may become more complex if a research group is actually forced to cross disciplinary boundaries. In the case of the selective surfaces research program, two different professional orientational reference groups emerged - science and engineering. Although the physicists' primary allegiance remained to science, which is more inwardly directed towards hegemonised stocks of knowledge, the fact that the group became increasingly confronted with problems that they categorised as engineering-type problems and not actually physics (for example, the design of a heat transfer system for the solar collector and problems involved with the mass production of the collector system) meant that either engineers were consulted and sub-contracted, or that some of the time of the scientists was spent contemplating problems that were quite new and very difficult (not all of the scientists took up the challenge with equal enthusiasm). The upshot was that a new reference group which was much more directed towards clients with practical needs and who would become only the vicareous users of the physicists' hegemonised knowledge and skills began to gain significance: that is, an engineering professional orientational reference group.

The complexity of this "double bind" has been compounded by the involvement of the group members in unfamiliar areas of political economic strategy: because of the group's involvement in the process of technological innovation, the types of exchange that the group have become involved with have increased beyond the "normal" routines of public addresses, conferences, publication in reputable journals, and so on. For example, as a relatively natural consequence of becoming involved in the process of technological innovation, the ideas embodied in the collector and its process of production have been protected against "theft" through a process of patenting.[13] Through the patenting of ideas the group members of the selective surfaces program have in fact increased their own value (in the eyes of their colleagues, administrators, and potential funders), for in addition to the group being valuable as a resource of knowledge and skills, the group now *owns* ideas in an economic sense (as opposed to the "intellectual" ownership attached to publication and, more occasionally, eponymy). This legalised economic ownership may be more secure (and "negotiable") than the apparently less tangible cognitive possession of rare craft skills and arcane knowledge, but still hardly sufficient to guarantee the perpetual security of any scientist (qua scientist).[14] What remains as job security is, of course, the typical professional assets of position, accomplishment and respectability (that is, professional status and power).

These are the main institutionally defined components of the "double bind" situation that constrains the physicists in the selective surfaces program. Much more could be said about the general context of this double bind: the effects of an economic recession, Liberal Government "science policy" (thoroughly laissez faire in its lack of policy), research funding arrangements in Australia, the obvious social, economic and political mediation of most Australian research (for example, funds for the SSP came from a Labor Party N.S.W. State Government and the Saudi Arabian State - the effects of this funding mix on the SSP remain to be seen however), the popular ambivalence about particular events in the historical emergence of physics in the twentieth century, and so on. Some of these subjects have already been dealt with as part of a general analysis of the directed nature of science in Chapter 3, but a more detailed historical, political and economic analysis is beyond the scope of this thesis.

Considered more generally in the light of the discussions in Chapter 4, the "double bind" is just another consequence of a highly specialised division of labour in a capitalist society, and from that point of view hardly a startling discovery. What is more startling are the effects that this separation of relevancies has had on a particular research program. In particular, the separation of a "context of research" from a "context of legitimation" will be dealt with in subsequent sections where amongst other things, the existence of a relatively autonomous "solar ideology", the stratification of research goals into a more "general" socially, politically and economically mediated level (which tended to be imposed from "above" and excluded from the domain of research) and, a more technically oriented level which was the "real" substance of research so far as the researchers were concerned will be discussed. The analysis which follows is intended to strike at the heart of the cognitive structuring of the selective surfaces program in such a way as to expose typical characteristics of a finite "sub-universe of meaning" in the natural sciences. In other words, it seems hardly contentious at this stage in the argument to now take the kinds of conflicts discussed so far for granted. The main focus of this chapter is in fact, more specifically oriented towards the particular mode of institutionalisation of the research program - what are the various structures that define the two contexts of the program and how do they relate to processes of research on the selective surfaces program?

6.6 Solar energy beliefs in the context of legitimation

Summary: Researchers in the SSP tended to bracket social considerations (including political and economic aspects) about their research as

"external" to the research process. As a consequence of this the more social aspects of their research were perceived by the scientists as part of a Solar Energy Belief System (H3).

This belief system is a body of ideas about solar energy which is shared by most solar energy researchers and which formed the major component of the context of legitimation of the SSP (H4). The Solar Energy Belief System formed part of the metaphysical level of the cognitive field of the members of the SSP (H6), but because of its more social orientation did not provide significant relevancy in the context of research (H7). The institutionalisation of this belief system is associated in the case of the members of the SSP (and solar energy researchers in general) with scientific marginality (H15).

One of the major distinguishing characteristics of scientists who are seriously involved with solar energy related research is that they generally share a body of ideas about solar energy which tends to be cognitively and socially separated from the theory and practice of day-to-day research. As a legitimating device, this belief system functions typically to keep outsiders "out" and insiders "in" [Berger and Luckmann, 1967:105]. That is, the solar energy belief system described in this section particularly emerged in "external" social contexts in which scientists were concerned with the discussion or legitimation of their research outside of an "in group" context which was dominated by the research goals and theoretical structures described in the next section.

In this I have not attempted to provide a detailed analysis of how the belief system functioned in the day-to-day activities of the researchers - thus, for example, I have not attempted to provide historical details of just how and when the context of research functioned as a "safety valve" for the particular researchers concerned [cf. Section 4.7]. Let us though be quite clear what the primary empirical claim is. On the basis of the material presented in this chapter I am suggesting that solar energy research in general is sufficiently cognitively institutionalised to enable the identification of specific ideas (including "beliefs" as identified below) which persistently recur in particular communicative contexts and which tend to occur irrespective of the particular type of solar energy research being conducted. In addition, it is also being claimed that these ideas tend to be, in the minds of the particular Australian researchers discussed in this paper, separated from research practices - cf. Sections 4.8 and 5.4-2. In other words, all the members of the SSP either articulated or implied all the components of the Solar Energy Belief System over the course of the interviewing program. Of course solar energy specialists are not the only scientists who share beliefs about solar energy, but as is the case for any specialists, beliefs and "knowledge" in the specialised domain (or "sub-universe") tend to be institutioanlised in a particular form that reflects collective interests - social, political, economic, as well as theoretical and technical interests. Finally, as discussed in Section 4.8, this belief system is merely an indicator of this context of legitimation - in real life situations it is taken for granted that other goals and beliefs will also be present in the context of legitimation as a function of particular individuals and organisations (for example, particular goals may become displaced from the context of research to the context of legitimation as will be briefly discussed in this section and again in Section 7.10).

On the basis of a literature search and programs of formal and informal interviewing with Australian and European solar energy researchers, the beliefs listed in Table 6.6-1 are proposed as a structure which is widely shared by solar energy researchers. The beliefs form an important article of *faith* (well grounded as the beliefs may be) about the relevance, importance and potential of solar energy research. The system is probably international in scope, but the sources used restrict its empirical validity to Western capitalist nations. As with any belief system the process of sharing (even if it is restricted to the level of beliefs) provides the basis for group solidarity and the development of a "community of interest" (but in the present case of solar energy research, as argued earlier, not for the development of a specialty). The beliefs also function as a legitimation for a range of similar pursuits - they are some of the "good reasons" why solar energy research should continue to be performed.

Contrasted with the beliefs of solar energy specialists are firstly beliefs of a "traditional energy community" of management and research workers in the "mainstream" areas of energy generation (that is, the areas of coal, oil, gas, nuclear and hydro-electrically based energy generation) and secondly, a "radical view" based on the view of those committed in various ways to the overthrow of the existing mode of energy production. This "radical view" is presented largely as a point of reflection about some of the possible implications of solar

<u>TABLE 6.6-1</u>: <u>Solar Energy Belief System</u> (1953-1979, approx.)

<u>Consensual high level goal implicit in the belief systems</u>: To make the sun work for humanity by extracting large amounts of useful energy from solar radiation.

	Substantive components of belief systems	Solar Specialists*	Traditional Energy Community*	A Radical View
1.	Solar energy can be in the long term be a significant contributor to global energy supplies (i.e. in more than 10 years)	+	±	+
2.	Solar energy has most potential as part of a "pluralist" energy system (including nuclear as well as fossil fuel based systems)	+	(+)	-
3.	Limited fossil fuel supplies require the development of solar energy as renewable resource consuming energy industry	+	±	+
4.	Solar energy generation can be effective with a minimum adverse effect on the environment	+	(+)	+
5.	Solar energy could make a significant impact on most natural energy supplies in the short term (i.e. within the next ten years)	+	-	+
6.	Solar energy is important as a potential source of low grade heat (i.e. at temperatures less than 300°C)	+	±	+

TABLE 6.6-1 (cont.)

	Substantive components of belief systems	Solar Specialists*	Traditional Energy Community*	A Radical View
7.	Solar energy will not be important as a source of high grade heat (and will not therefore have a direct role to play in the generation of electricity by conventional means)	±	+	-
8.	Solar energy can uniquely provide valuable specialist applications	+	(+)	+
9.	As time goes by solar energy will become more economically viable	+	(+)	+
10.	Any deficits in knowledge about solar energy and its utilisation are easily surmountable	+	(-)	+
11.	The main knowledge deficits are scientific	-	(-)	-
12.	The main knowledge deficits are technological	±	+	+
13.	Increased investment in solar energy related research and development is required	+	(-)	+
14.	Solar energy research can be part of a radical scientific or social program	(-)	(±)	+

KEY:
- \+ Strong positive consensus*
- \- Strong negative consensus
- ± Ambivalence
- (+) Tacit positive consensus
- (-) Tacit negative consensus

* These entries are based on a survey of 103 publications and 174 authors [in Jagtenberg, 1975]. The primary sources are listed below. 63 of the publications involving 134 authors were directly related to solar energy research. But as discussed in the text, this literature survey is only one level of the evidence.

The judgements about consensus and ambivalence were quite easy to make: strong positive or negative consensus means that these beliefs were explicitly stated by the authors surveyed without contradiction from "fellow" researchers (either in the literature surveyed or in my subsequent interaction and reading). Ambivalance means that conflicting views were expressed explicitly and implicitly. A tacit consensus means that whilst views were not explicitly expressed they were taken for granted - as might be expected, this involved less direct means of assessment. The numerical details of the "polling" of the literature are given in Table 6.6-2 which follows. As mentioned however, these numbers are only part of the picture and should be considered as heuristic in nature.

<u>Major sources</u>

F. Daniels and J. Duffie. <u>Solar Energy Research</u>, Madison: University of Wisconsin Press, 1955 (based on a Symposium largely sponsored by the U.S. National Science Foundation, Madison, Wisconsin, September 12-14, 1953).

<u>Proceedings of World Symposium on Applied Solar Energy</u> (Symposium sponsored by the Association for Applied Solar Energy, Phoenix, Arizona, November 1-5, 1955), Phoenix: Association for Applied Solar Energy, 1955.

P. Donovan, et.al. <u>An Assessment of Solar Energy as a National Energy Resource</u>, Report prepared for the U.S. National Science Foundation and National Aeronautics and Space Administration, Report No. NSF/RAIN-73001; Springfield: National Technical Information Service, 1972.

<u>Report to the Committee on Solar Energy Research in Australia</u>, Australian Academy of Sciences, Report Number 17, September 1973.

"Solar Energy", Memorandum by the International Solar Energy Society to the Select Committee on Science and Technology (Energy Resources Sub-Committee), Part 1, Session 1974-75, HMSO, January 22, 1975, Number 156-i.

TABLE 6.6-2: Breakdown of the raw data in the literature survey of solar energy specialists contained in Jagtenberg [1975].

Component of the solar energy belief system	Type of consensus indicated in Table	Number of authors explicitly stating beliefs		
		+	-	% of sample
1	+	6	0	4
2	+	79	0	59
3	+	78	0	58
4	+	77	0	57
5	+	6	0	4
6	+	94	0	70
7	±	66	15	49/11
8	+	22	0	16
9	+	96	0	72
10	+	100	0	75
11	-	0	85	63
12	±	96	4	72/3
13	+	79	0	59
14	(-)	0	0	0

Total number of authors "polled" in literature survey: 134.

energy research and is suggested as a logical entailment of the belief that solar energy can provide the means of overthrowing a mode of energy production based on the consumption of non-renewable energy resources. The beliefs of the "traditionally" oriented status quo in the field of energy generation were constructed on the basis of a much more restricted literature search but are grounded in my own broadly based experience of the fields of energy research and management. The "radical view" is presented more as an "ideal type" than as a representative of any well established community of interest. It is however, based on experiences with a range of people who have self professed "alternative" life styles; these experiences have been through a literature devoted to "alternative" technology and life styles [15] and through personal contacts over the years. The information presented in the "radical view" has not been collected with quite the same methodological rigour as that in the solar ideology, but the "radical view" is still empirically well founded. [16] Important as questions of empirical validity are, we should not though lose track of the reasoning behind this concern to contrast the solar research energy belief system with two rather more impressionistic sets of beliefs: no set of beliefs, or ideology, exists in a cultural vacuum, and before sense can be made of something called a solar energy belief system it is essential to see how it compares against competing beliefs (and the interests that they represent).

This marginality of solar energy research, coupled with the tendency towards absorption (or "incorporation") [17] into the scientific mainstream and industrial mode of production that all modern science demonstrates, explains the apparent liberal pluralism of most solar energy researchers, particularly those who are more concerned with their future livelihood. Beliefs 2, 6 and 7 express this more modest side of what is considered possible. Enthusiastic though most solar energy researchers may be about the possibilities of solar energy systems, such enthusiasm is well tempered by an awareness of the likely outcome of a direct struggle with energy monopolies.

One might wonder why these beliefs should "crystallise" when other areas of scientific research appear to proceed without such obvious support. That is, how can we account for the relatively high level of institutionalisation of the context of legitimation of the SSP when compared with the rather diffuse metaphysical beliefs of the researchers in the DOP, for example?

As we will see, solar energy research is a relatively value-laden area and researchers appear to expend rather more than the normal amount of energy proselytizing in a wide range of social contexts. This is particularly true of the more elite researchers who tend to be more involved in the political and economic aspects of the struggle for scientific stakes. The best explanation for this apparently high level of ideological activity and the associated institutionalisation of a coherent set of beliefs amongst solar energy researchers seems to me to rest in the marginality of solar energy research (as defined in Section 6.4). This marginality (which is somewhat higher than the marginality of the DOP [see Table 8.3-6]) together with the typical struggles associated with all marginality provided sufficient reason for the emergence of a well defined system of beliefs. Functionally speaking, this system of beliefs is one of the weapons necessary for survival, consolidation of existing forces, and (given success) the inevitable expansion of a research field.

If all the beliefs listed above are considered together one overarching goal emerges. The essence of this goal is one of "making the sun work for humanity by extracting large amounts of useful energy from solar radiation" [cf. Jagtenberg, 1975:184, 185]. This goal doubtless still has some influence on the day-to-day research of solar energy researchers, but given that the goal and its implicit components are highly dependent on social, economic and political factors of a very general nature (for example, energy policy at a national and international level), it did not have much relevance to the theoretical and technical issues which largely constituted the field of day-to-day research of the members of the SSP (as will be described in the next section). There can be little doubt that the early pioneers of solar energy research - inventors, amateurs, and the odd reputable scientist and engineers, were directly inspired by this goal, [18] but as the field of research became more highly institutionalised and academically oriented the goal increasingly became, for most researchers, a device for the legitimation of a marginal field of research. That is, the goal of "making the sun work for humanity by extracting large amounts of useful energy from solar radiation" has become displaced from a context of research to a context of legitimation. [19] In this context the goal and its implicit components (as expressed in the solar energy belief system) serves to elicit the mass loyalty of solar energy researchers and all those interested "outsiders" who wish to share a socially legitimate goal; in the context of legitimation the goal also furthers the avoidance of full participation in projects that have the goal as a primary concern [see Section 4.7].

As a legitimating device, the belief system is not without internal contradictions - this is, as Marxists would point out, necessarily in the nature of all ideologies. The high level goal of "making the sun work for humanity . . ." is particularly compromised by the two lower level goals which tend to preserve solar energy as marginally useful resource compared to other energy resources - viz., "Solar energy has most potential as part of a "pluralist" energy system (including nuclear as well as fossil fuel based systems)" - component 2 in Table 6.6-1; and, "Solar energy will not be important as a source of high grade heat (and will not therefore, have a direct role to play in the generation of electricity by conventional means)" - component 7 in Table 6.6-1. Such contradictions did not however, appear to detract from the major social function of the belief system; that is, the production of group solidarity.

Finally, although it is taken for granted that the "interested" promotion of beliefs will involve, at some level, communicative distortion (for example, the selection and exaggeration of information as discussed in Section 7.11), I am not in any position to pass any extreme negative judgements about the Solar Energy Belief System presented below. Quite the contrary in fact; it may be that history will demonstrate the shortcomings of some of these beliefs, but on the basis of present information, a good case can be made for most of them. 20

This belief system is ideological in function insofar as it marks a separation between theory and practice in scientific work. In the present case it is quite clear that the researchers involved were conscious of a "paramount" scientific reality which was based in day-to-day practices: the "context of research". This context of research appeared to be relatively free of considerations raised in the belief system: what was most relevant in this context was the task at hand, the quality of research, its success or failure, and other related "task oriented", "internal" issues. [21] The relative narrowness of this task orientation is amply demonstrated by the technical orientation of the goals that dominated the selective surfaces program - this issue will be further developed in the next section.

6.7 Goals and theory in the context of research

Summary: *Although many of the more overtly social and political aspects of solar energy research formed part of the solar energy belief system which was separated from the context of research (H3), the members of the SSP were influenced in their research by issues related to the development and application of their research (H14). This research occurred in the context of a structured cognitive field which consisted of interpenetrating theoretical, subject concern and technical levels. Two structures have been identified on the basis of empirical data: a theoretical landscape and a constellation of goals. These structures have been stratified into a disciplinary, sub-disciplinary and program level of research - the program level being more highly concerned with specific aspects of the theoretical, subject concern, and technical*

levels of the cognitive field (H6). These structures provided relevance for research (H7) and were oriented towards two different provessional orientational reference groups, one scientific and the other engineering in type (H1). The research goals of the members of the SSP were numerous and occurred at different levels of the cognitive field of the research program (H8).

The various structures presented in this chapter (the solar energy belief system, the theoretical landscape and the constellation of goals) are being presented in an attempt to describe particular cognitive structures which taken together could be considered as largely constituting the cognitive "field" of a group of scientists - that is, the field of possibilities and constraints which in conjunction with particular intended objects largely constituted individual consciousness in the contexts of research and legitimation.

In contrast to the Solar Energy Belief System the "theoretical landscape" presented in Table 6.7-1 is a structure of specialised knowledge that formed the major part of the theoretical background for the research of the scientists involved in the selective surfaces research program. Much of the theoretical (and technical) knowledge which became relevant to the research program emerged from these horizons. For example, the goals (or objectifications of the in-order-to motive) of the researchers are largely projections from this same landscape.

The theoretical landscape in Table 6.7-1 has been stratified into three levels: disciplinary, sub-disciplinary and program levels, as defined in Chapter 4. The components of the landscape in Table 6.7-1 are also ordered according to generality and professional orientation: as one proceeds down the Table there is a movement towards the specific theoretical relevancies of the particular research tasks that constituted the day-to-day research on the program, and as one moves across the Table there is a change in professional orientation from science to engineering. It is worthy of note that there are no less than seven different sub-disciplinary components in this theoretical landscape. This spread is an indication of the way that the pragmatic demands of practice orientation force even the theoretically inclined into diverse areas. It also indicates the way that any research program must be conceived as a region of *intersection* between different scientific sub-universes and not as mental events in splendid isolation.

Except for a minor shift in the professional orientation of three components the second round synthesis of the structure was accepted by the program members as an adequate description. This surprisingly rapid convergence is taken as an indicator of a fairly high level of cognitive institutionalisation: the scientists had no difficulty in identifying areas of particular theoretical (and technical) relevance to the program as a whole and to their own efforts in that program.

TABLE 6.7-1: The theoretical landscape of the SSP (as of April, 1977).

Level of scientific sub-universe	Components of the theoretical landscape and their professional orientation*		
	Scientific		Engineering
Discipline	T1 Physics		
	T2		Mechanical Engineering
Sub-discipline	T3 Solid State Physics		
	T4 Materials Science		
	T5 Thin Film Physics		
	T6	Vacuum Technology	
	T7	Heat transfer	
	T8 Selective Surfaces		
	T9	Solar energy utilisation	
Program	Properties of:		
	T10 Geometric selective absorbers: 10.1 metal mesh absorbers 10.2 globular metal films		
	T11 Cermet Surfaces		
	T12 Graded Interference Layers		

* The components have been listed under the profession towards which research workers in a particular sub-universe are most likely to be oriented in different aspects of their research. Where both professions are influential the components have been listed in the centre of the Table.

It must be stressed that whilst this theoretical relevance structure has been accepted by the scientists concerned as a "reality" it is not being claimed that this landscape is identically perceived by individual scientists. Different individual perceptions were expressed through individual scientist's different priorities and different levels of involvement with the various goals that considered together give the program the character of "projects of action" [see Section 4.4]. These goals have been listed in Table 6.7-2 and the scientists' priorities, levels of involvement and "autonomy" are dealt with in subsequent Figures. The subject of differing individual perceptions will be pursued in the discussion of these Tables and Figures.

The goals listed in Table 6.7-2 have been arranged in the same way as the components of the theoretical landscape: as one proceeds down the list there is a tendency towards more specific research goals, and as one moves across the Table there is a change in professional orientation from science to engineering. Movement down the Table towards more specific goals has been subdivided in terms of "level of applicability". That is, some goals are more general in their possible field of application, being significant not only to a particular research program, but also across various sub-disciplinary concerns. This does not preclude the goals listed at the program level from having a wider relevance than the particular program under consideration; rather, it is implied that at the program level these goals tended to be uppermost in the minds of the researchers as they pursued their day-to-day tasks. In one sense then, this is an

TABLE 6.7-2: Theoretical and technical goals that effected the direction of research on the SSP (as of April, 1977).

Level of applicability of theoretical and technical goals	Professional Orientation* (Scientific — Engineering)	Approximate date of emergence of goal as relevant to research
Sub-discipline	G1 Establish a solid state physics research front	Oct., 1973
	G2 Establish a solar energy research front	Oct., 1973
	G3 Establish a solid state oriented solar energy research front	Dec., 1973
	G4 Develop an alternative to the established flat plate collector	Jan., 1974
	G5 Incorporate selective surface research with solar energy research	Jan., 1974
Program	G6 Find a new, efficient selective surface	Jan., 1974
	6.1 Develop a refractory selective surface	May, 1975

* The components have been listed under the profession towards which research workers in a particular sub-universe are most likely to be oriented in different aspects of their research. Where both professions are influential the components have been listed in the centre of the Table.

TABLE 6.7-2 (cont.)

Level of applicability of theoretical and technical goals	Professional Orientation: Scientific — Engineering	Approximate date of emergence of goal as relevant to research
Program (cont.)	G7 Develop a system which produces heat in the intermediate range of 100-300°C	Nov., 1974
	G8 Develop a commercially viable collector which employs the new selective surface	Nov., 1974
	G9 Develop a model of the collector system	May, 1975
	G10 Investigate the properties of geometric selective absorbers:	
	10.1 metal mesh absorbers (i.e. grids)	Oct., 1973
	10.2 globular metal films	May, 1975
	G11 Investigate the properties of cermet surfaces	May, 1975
	G12 Investigate the properties of graded interference layers	May, 1975
	G13 Develop ways of applying selective surfaces to substrates	Nov., 1974
	13.1 Develop the sputtering technique	Nov., 1974

TABLE 6.7-2 (cont.)

Level of applicability of theoretical and technical goals	Professional Orientation			Approximate date of emergence of goal as relevant to research
		Scientific	Engineering	
Program (cont.)	G14		Develop an evacuated tubular sputtered glass collector	Nov., 1975
	14.1		Develop a long sputtering chamber	Nov., 1975
	G15		Develop commercial applications for a working solar array	Nov., 1975
	15.1		Develop a heat extraction system	April, 1976
	15.2		Develop a configuration of collectors and concentrating mirrors	Dec., 1976
	15.3	Check degradation mechanisms of the materials involved		Nov., 1975
	15.4		Develop an air conditioning system using the solar output	Dec., 1976

attempt to deal with the phenomenology of task orientation.

All the goals listed received consensual validation from the scientists involved. This validation was, as discussed in Section 5.4, a consequence of the methodology used, whereby the goals that were initially nominated by me were modified by the researchers in a process of negotiated "feedback". As with the theoretical landscape the rate of convergence over the goals of the program was surprisingly rapid. Consensus was achieved with the second version of the list of goals in Table 6.7-2. However, the same remarks about different individual perceptions apply here too. The fact that a list of goals can be agreed upon does not mean that the goals mean the same thing to all concerned. [22]

Both these structures are similar in their function of providing criteria of relevancy for researchers. Neither of the structures uniquely provide any one kind of relevancy (that is, motivational, or thematic, or interpretational relevance), but, considered together as part of an overall process of research, it is clear that they provide at different times and in different contexts all three kinds of relevance. The constellation of goals which exists on the basis of various theoretical interests and assumptions explicit in the theoretical landscape is an expression of historically sedimented themes of research, as well as expressing a compressed theoretical "background". This landscape, in conjunction with the constellation of goals, provided both thematic and interpretational relevancy for the researchers. For example, given a particular goal and a particular situation to be interpreted in the light of particular information, that information gained interpretational relevance by virtue of its relationship to the components of the theoretical landscape and other goals which acted as filters in the theoretical landscape. The inter-related functions of these structures will become further apparent in the next section where the process of research will be described in considerable detail showing how the goals, which are projections from within the theoretical landscape are related to a process of research.

The experience of actually generating this list of research goals from the information volunteered by the scientist subjects strongly supports the notion that the world of research in the selective surfaces program was a semi-autonomous and very finite province of meaning and further supports the hypothesis that scientists tend to bracket social considerations about their research as "external" to the research process. The "logic" of problem development which is reflected in the chronologically arranged list of goals [23] was immediately available from the scientists' "common sense" appreciation of the research program but it was noteable that this appreciation did not involve much social (or economic or political) analysis on the part of the scientists. That is, on the basis of a number of relatively undirected interviews specifically concerning the development of the research program a largely theoretically and technically oriented story emerged. Social, political and economic analysis was not a priori excluded from the scientist's elaboration of what was important for an understanding of the research program - in fact, on occasions it was actually encouraged. Even so, after one and a half years of contact with the members of this research program the sum total of my interactions indicate that a very clear separation had been affected by the scientists between a sub-universe of research which was theoretically, technically and socially oriented (that is, the "context of research") and the rest of the members' lives.

There is a sense in which this split in relevancies is artificial however, since the scientists questioned implicitly based their discussions on a "taken-for-granted" stratum of "non scientific" (i.e. social) factors which made sense of the logic of the program's development. These "non-scientific" factors tended to come up as explanations only if I explicitly directed conversations (which were initiated on the basis of "tell me about your research") towards other people's roles. The fact that this "stratum" was taken for granted and "bracketed" as non-scientific (and therefore not very interesting or of much explanatory power) is supported by the way in which a list of "significant" social factors involved in the establishment and evolution of the goals of the program [24] was accepted almost without comment by the scientists the very first time they were confronted with the list - by contrast it will be recalled that two rounds of negotiations were necessary in working out the theoretical landscape and constellation of goals. The reason for the scientists' apparent lack of interest in social analysis may conceivably have been due to a number of factors. The summary that the scientists were asked to examine may have struck them as exceedingly dull, or what is more likely, it might have been defined by them as "sociology" and therefore best left as the sociologist's game - this is, of course, a response that one might expect from any well trained professional. Or the avoidance could have been due to some political instinct which reacted to sensitive areas. Or it could have been due to the busyness of the scientists and their reluctance to begin more time consuming discussions. All these are possibly valid explanations. I am convinced however, that the apparent lack of interest that the physicists had for social analysis more accurately reflected a feeling that this "stuff" was simply not in the context of research; important background, maybe, but certainly not of much immediate importance. This is particularly apparent in the interview material. When subjects were left to talk about their research in a fairly undirected way social, political and economic relevancies were rarely voluntarily raised as important considerations. Even when I did raise such "external" considerations they were mostly avoided or fairly superficially treated. That is, the world of the research program was for the scientists most definitely a *finite* sub-universe of meaning whose horizons might involve some social and economic considerations, but whose lived experience appeared to be dominated by physics oriented theoretical and technical relevancies. This "blinkering" is, after all, what specialisation and task orientation entail for most professionals [25] [see Section 2.4].

The only major exception to this physics oriented cognitive internalism of our solar energy researchers stemmed from their concern with the goal of developing a commercially viable collector (G8 in the constellation of goals). Here the particularly important problems involved with the economy of mass production and the existence of competing sources of energy were well appreciated and even incorporated as part of the overall research strategy. Goals 7, 9, 14.1, 15 and 15.4 derive directly from the awareness of an economically mediated horizon of possibilities - these goals reflect on awareness of the thermal requirements of industry, the need to produce a device which can be mass produced without major design changes, the need for a device with a range of potential applications, and the need for developing laboratory production techniques which can be readily adapted to the demands of mass production. All these considerations are consistent with an "engineering approach" to the problem of solar energy conversion and directly support the postulation of the importance of an engineering professional orientational reference group in accounting for the development of the research program.

The fact that social analysis was generally bracketed out of the context of research by the scientists does not mean, though, that social factors were actually unimportant in the context of research. I have attempted to focus more specifically on the social dimension of the research goals in Section 9 of this chapter.

First however, I propose to more specifically describe the processes of research in the SSP as constrained by one particular structure of relevancy - a constellation of goals which became sedimented over a period of time.

6.8 Goals and the evolution of research on the selective surfaces program

Summary: *The members of the SSP were constrained in their research by an evolving constellation of goals which provided a structure of*

relevancy for the scientists (H7). The goals of the scientists did change over time but this change occurred as a process of sedimentation of new goals into established structures rather than by the replacement of old goals (H10).

The goals of any action are projections from particular contexts of events. Considered as such goals have an obvious historical dimension and relate to particular events which can be isolated from the ongoing flux of various projects of action.

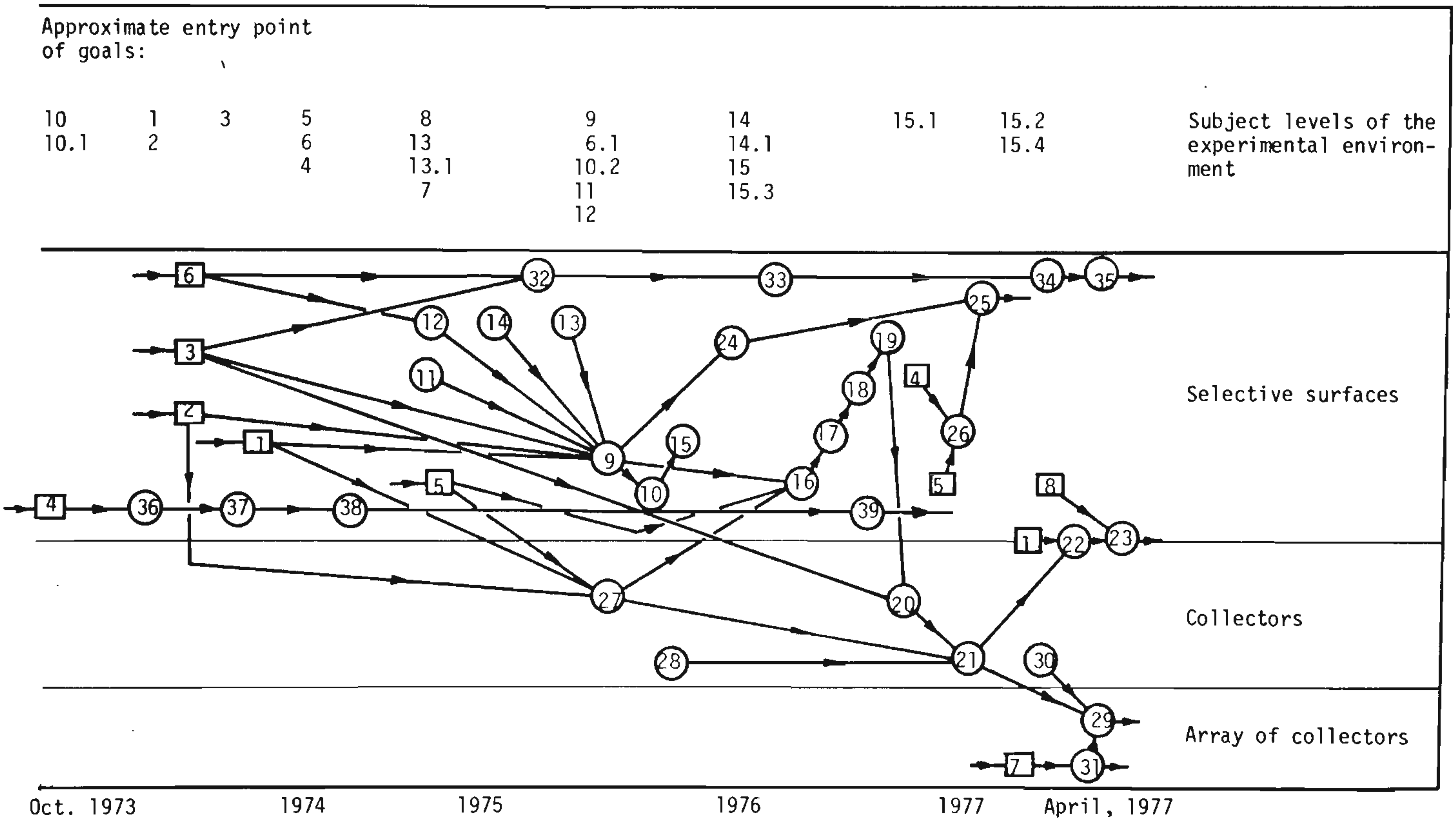
Figure 6.8-1, "A flow diagram of significant research events in the evolution of the selective surfaces research program" is an attempt to relate the goals listed in Table 6.7-2 to a process of research. The diagram pictures an evolving constellation of goals which provided a structure of relevancy (primarily in-order-to motivational relevancy) for the members of the SSP.

The direct connections that have been made between various events and individual lines of research represent *themes* of research. Implicit here is the idea that particular events can be seen as being important to the evolution of specific lines of events. This may be a vague approach to causation but anything more "precise" would probably take us too far away from the empirical material. I should add, furthermore, that as far as the individual scientists were concerned they were working in an environment which consisted effectively of a *flux* of action which was not pre-given as this or that "type" of event capable of causing this or that effect - that is, a precise knowledge of the kinds of relationships between the ideas, hypotheses, observations and findings which are lumped together on the diagram as "events" was certainly not a major interest of the scientists concerned - invariably the scientists were intrigued by (but never dismissive of!) the order that emerged from my interviews with them. It is certainly not being suggested that such order is merely a sociological artefact - that some structure existed is basic to the whole idea of an ordered cognitive field. Nonetheless, a very real problem associated with the diagram is that the order detected may be at the expense of the disorder ignored - as implied above most of the researchers were surprised that an apparently chaotic process involved such order (or perhaps they were amazed that anyone could be bothered in trying to recapture what was felt to be a loose process).

Indeed, the "exact" way in which one person's research affected another's is probably impossible to reconstruct; the best we can hope for is a "rational reconstruction" guided by some process of selection - in the present case the process of selection was rather necessarily based on a consensus which emerged from many related discussions [see Chapter 5].

In the diagram the goals are located at their approximate points of entry as directives for future research. The structure of goals is cumulative insofar as once a goal was perceived it remained as an objective for the group. There was only one exception to this generalisation about the cumulative nature of the goals: G5, "to incorporate selective surface research with solar energy research" was consensually felt to have been fully achieved by April 1977 (the cut-off point of the study) and so would have become of only residual significance by

FIGURE 6.8-1: Flow diagram of significant research events in the evolution of the SSP. (The key to the numbered research events follows overleaf).



KEY TO FIGURE 6.8-1: Significant research events in the evolution of the selective surfaces research program.

NOTE: For greater ease of comprehension this list has been arranged to demonstrate something of a logical development of events. This reconstructed logic is sometimes at the expense of the chronological sequence of events.

PROGRAM MEMBERS* AND THEIR ESTABLISHED LINES OF RESEARCH -

1. Solomon: Superconductivity.
2. Ulrich: Phase transfer mechanisms in non-conducting solids.
3. Neil: Magnetism.
4. Phillip: Metal mesh absorbers (Ph.D research).
5. Oscar: Theoretical research on metal meshes.
6. Walter: Graded interference layers (B.Sc.Hons. and Ph.D research).
7. Eric: Refrigeration (Ph.D research).
8. Robin: Sputtered films (Ph.D research).

PARTICULAR EVENTS -

9. Metal blacks were established as promising selective surfaces.
10. Research into the properties of gold and chromium blacks was undertaken.
11. Metal meshes were rejected as commercially viable selective surfaces on account of optical performance problems.
12. Emissometer developed.
13. Absorptometer developed.
14. The serendipitous discovery in 1952 of the selective nature of gold blacks was brought to attention by an old colleague.

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* Pseudonyms have been used.

KEY TO FIGURE 6.8-1 (cont.)

15. Research into the properties of gold and chromium blacks was discontinued because of the higher priorities of other research.

16. A reactive sputtering technique was developed for a range of materials.

17. Oxides were rejected as selective surfaces because of their instability at high temperatures.

18. Metal carbides were established as prospective selective surfaces (but with the knowledge that there are no "magic" carbides).

19. A sputtered iron carbide surface was successfully deposited.

20. A sputtering chamber was constructed.

21. An evacuated, tubular glass collector with a sputtered iron carbide selective surface was constructed.**

22. Problems with the mass production of carbides were investigated.

23. The high rate deposition of sputtered surfaces was investigated.

24. Cermets were established as surface candidates.

25. Theoretical advances in cermet theory were made: the traditional approach in predicting optical constants from the Maxwell-Garnett formula was found to be in error in some cases. An alternative "exact" method using a technique invented by Lord Rayleigh was developed.

26. Certain program members found themselves in disagreement with a number of other workers in the field concerned with the correct theoretical treatment of cermets.

27. Flat plate systems were rejected as profitable from the point of view of research and development.

** Patents for an iron carbide selective surface and a process for deposition for the surface based on reactive "sputtering" were lodged in June 1977, by the three scientists most involved with the selective surface and its deposition.

There is also in existence a patent for a type of selective surface that was lodged in August 1976, by three Americans, but this is not as specific as the Australian patent since it covers a wide range of possible surfaces and does not refer to one particular surface. The legal position in the event of a property dispute is not clear.

KEY TO FIGURE 6.8-1 (cont.)

28. It was discovered that an American firm had independently developed an evacuated tubular glass collector. The possibility of using these collectors as the basis for the program's selective surface system remains as an option.

29. An array of collectors was assembled and demonstrated to the State Premier and the press.

30. A heat exchanger for the collector was developed.

31. An air conditioning device was proposed as a promising avenue of development.

32. The grading of the optical properties of a surface was hypothesised to be a potentially viable method of producing a selective surface.

33. Theoretical results suggested that grading had potential for improving selective surfaces.

34. Grading was shown to substantially improve the selectivity of iron carbides.

35. The structure of sputtered carbides was felt to be understood.

36. It was hypothesised that metal meshes might provide a surface with selective properties.

37. Particular metal meshes were developed.

38. It was hypothesised that globs might provide a surface with selective properties.

39. Particular globs were developed.

that time (but note that in the case of general goals, such as G5, it is unlikely that even when a goal is felt to be achieved that it ceases to remain as a goal for research - this "fading" from consciousness could only occur, it is suggested, in the case of the more concrete and technical goals such as goals 6 to 15.4). Furthermore, the stability of the emerging constellation of goals that is depicted on the flow diagram appeared to be very high. Only one scientist felt that his priorities underwent significant change over the period of the diagram and that change was reported on being a fluctuation of first priorities in the final year of the period of the diagram. Thus, the goals of scientists in the SSP did change over time, but this change occurred as a process of sedimentation of new goals into established structures rather than by the replacement of old goals.

From the point of view of the "final" product which was the desired outcome of the research depicted (that is, a commercially viable collector which employed a new selective surface; cf. G8) two events are of particular significance: the establishing of metal blacks as promising selective surfaces (event 9) and the discovery that an American firm had independently developed an evacuated tubular glass collector which could be directly incorporated into the final device (event 28). These two events were largely responsible for the particular form of the collector that the team eventually devised. In the diagram it can be seen that event 9 is in fact the most heavily focussed node receiving more inputs than any other event on the diagram. This is no accident given the very high priority accorded G6, "find a new, efficient selective surface". On the other hand, event 28 was a relatively serendipitous discovery, and not the outcome of a specific goal concerned with the discovery of directly useful commercially available collectors. The group had in fact been quite reconciled to developing their own tubular collectors. The importance of this discovery lay mostly in the encouragement it gave to the then nascent ideas about the particular form that the collector should take. By 1979 the group had eventually decided not to use the overseas product but to contract a local glass maker to produce a similar product.

The co-operation implied by the schematic connection of various events and individual scientist's lines of research proceeded in the context of a division of labour and a hierarchy of authority. This division of labour and relationships of authority have been investigated in terms of the influence of individuals and groups of individuals over the formation of goals, their priorities for goals and their levels of involvement in work towards the achievement of particular goals. Some of these results will be discussed in the next section.

6.9 Some important characteristics of the process of formation and evolution of the research goals

Summary: *Research in the SSP was constrained by social economic and political factors. This was demonstrated through an analysis of various social aspects of the process of formation and evolution of the goals of research of the program. Not all of the goals of research of the members of the SSP remained equally relevant over time. The more general goals which were partially established in the context of legitimation, and effectively imposed "from above", were subject to displacement from the context of research to the context of legitimation*

(H9 and H4) wherein their relevance to day-to-day research was diminished (H3).

The constellation of goals which oriented researchers in the SSP was quite stable over the period examined. The goals were not subject to sudden change as a consequence of the emergence of significant research events. On the contrary, the constellation of goals was a cumulative structure of gradually sedimented goals (H10). The research of the members of the SSP did however, become increasingly technical in orientation as the program evolved (H11).

As shown in Table 6.9-1 the goals of research of the SSP evolved in the context of social, political and economic factors which were significant in influencing the content of individual goals and in providing a broad structure of motivational relevance for researchers in the SSP. (The primary data for this Table is contained in Figure 4 in Appendix 3). The social factors listed fall into two main categories: political/economic strategies primarily at the School and Department level of organisation within the university, and considerations relevant to career and personal advancement. Some goals were influenced by both types however. As shown in the Table the various goals were not equally influenced by all the factors. This is not to imply that particular factors were important in the formation of some goals and not at all relevant to the formation of other goals. On the contrary - there is a sense in which all the socially oriented factors provided a general matrix of relevance for all of the goals. The main criterion is that of relative degree of effect rather than

TABLE 6.9-1: Summary of socially oriented factors which were particularly significant in the establishment and evolution of the goals of the SSP.

	Significant socially oriented factors*	Goals (in approximate order of evolution)**																			
		10.1	10	1	2	3	5	6	4	8	13	13.1	7	9	6.1	10.2 11	12	14 14.1	15	15.1 15.2 15.3	15.4
A. Political/Economic strategies primarily at the School and Department level of organisation within the university	1				x	x	x		x	x			x								
	2(i)				x	x															
	(ii)				x	x			x												
	(iii)				x	x				x											
	(iv)				x	x															
	3			x	x	x															
	4(i)				x	x															
	(ii)				x	x															x
B. Considerations relevant to career advancement/personal advancement	5(i)					x	x	x													
	(ii)					x	x	x					x								
	6				x	x		x	x				x								
	7												x								
	8	x																			
	9								x												
C. Both of the above types of factors strongly involved	10													x				x			
	11												x								x
	12												x								x

* See Table (cont.) for a list of these numbered factors.
** See Table 6.8-2 for details of the goals.

TABLE 6.9-1 (cont.)

Significant socially oriented factors listed in the Table

A. Political/economic strategies, primarily at the School and Department level of organisation within the university.

1. Research with social and economic potential was considered to be valuable and worthy of pursuit. This belief has a general level of reference insofar as it is directed to all research and a particular level of reference insofar as it is directed to research on the selective surfaces program (as expressed in the solar energy belief system).

2. Research with social, economic and political potential was considered to be useful for -

   i. the further legitimation of the discipline of Physics;
   ii. the further legitimation of the School of Physics;
   iii. the raising of funds for further research;
   iv. attracting students to combat shrinking enrolments.

3. A gap in the research capacities of the School was perceived.

4. A goal which could be shared by other researchers in the university might provide a basis for increased solidarity between relevant Departments and Schools. This sharing could provide a basis for -

   i. collaborative research programs for established scientists;
   ii. the generation of Ph.D projects.

B. Considerations relevant to career advancement/personal advancement.

5. This goal was seen to be potentially fruitful from the point of view of the production of -

   i. publishable results (i.e. "respectable" physics);
   ii. a novel product worthy of being patented.

6. A successful program might provide economic returns to individual scientists and the School of Physics through royalties and consultancies.

<u>TABLE 6.9-1</u> (cont.)

7. An established line of fruitful research might be profitably expanded.

8. A potentially fruitful Ph.D project might be profitably expanded.

9. Direct competition with established programs of research is to be avoided wherever possible.

C. Both of the above types of factors strongly involved:

10. Successful research may require "development".

11. Existing economic market demands can be capitalised on.

12. Direct competition with State generated electricity is to be avoided where possible.

uniqueness of effect - as stressed in the theory section, research occurs in a broad social context and is best understood in this context. That is, Table 6.9-1 indicates a broad structure of motivational relevance for the pursuit of particular goals. This structure will be discussed further in the sub-section which follows.

Within this context the structure of goals shown in Table 6.7-2 was a very stable structure - none of the goals were said to have changed over the period, and the priorities of the researchers remained very stable over the period investigated.[26] No upheavals in research priorities were reported and the picture appears to be one of a *cumulative* structure of goals being gradually sedimented. Furthermore, as Figure 6.9-1 indicates, most of the goals are associated with a very steady rate of progress. This stability was associated with a number of features:

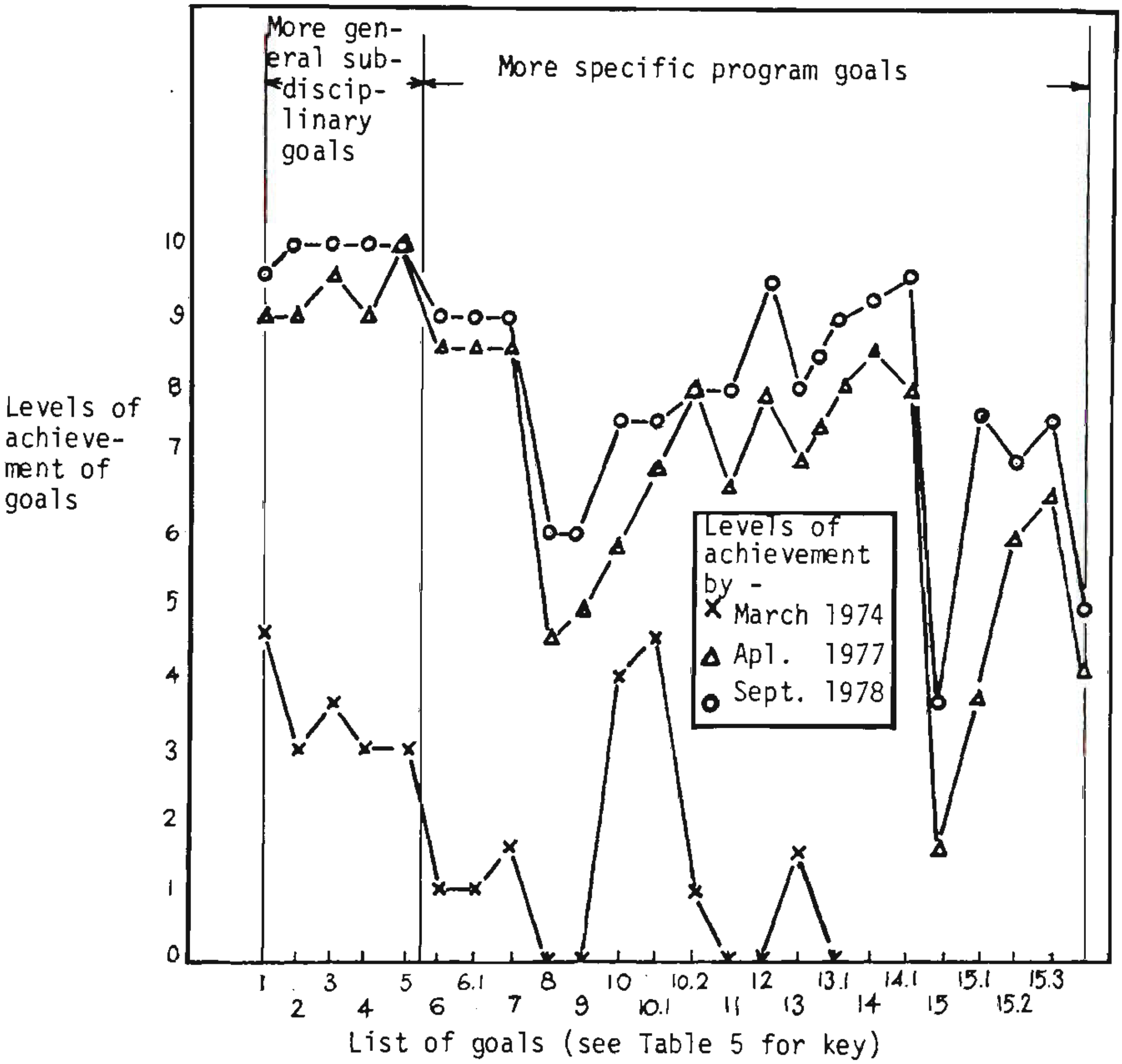
(i) The more general goals were partially established in the context of legitimation and partially in the context of research. Over time the general goals became relevant to research, however, since these goals were subject to displacement from the context of research to the context of legitimation -

The more general goals of the program were all established within the first year [see Figure 6.8-1] and as Figure 6.9-1[27] indicates, these goals are associated with higher levels of achievement than the other goals. The *rates* of progress (that is, overall changes in levels per unit time) towards the more general goals are not significantly different however [see Appendix 7]. Although the program was originally formed in the context of ongoing research (goals 10 and 10.1) the importance of establishing some broader guidelines for future research are indicated by goals 1-6 in particular. There can be no doubt that one of the main functions of these goals was that of justifying the establishment of a semi-autonomous research group. This legitimation process had two important aspects - firstly, legitimation to "significant others" who might be needed to provide economic, political and scientific support, and secondly, "internal" legitimation involved in the process of the formation of a research group that felt itself to have an identity (that is, intellectual and social coherence - not that these two aspects are fully separable).

Furthermore, as shown in Table 6.9-1 these first six goals were agreed by the researchers to be less concerned with matters that were immediately relevant to career advancement and personal advancement than were some of the other goals. That is, although the motivations involved with the more general goals were not completely separable from narrower ego-based political and economic considerations, goals 1-6 were felt to be, in comparison with other goals, more generally associated with issues of long term strategy. These issues which are summarised below, were then, not necessarily based in the immediate interests of the members of the selective surfaces program.

1. Research with social and economic potential was considered to be valuable and worthy of pursuit. This belief had a general level of reference insofar as it was directed to all research at the program level. The belief is also incorporated in the solar energy belief system however.

FIGURE 6.9-1: Levels of achievement of the goals of the SSP (group averages)*.



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* Note: These graphs have been consensually accepted as accurate representations of the levels of achievement of the goals. The mean deviations of the first round responses do however, give some indication of the extent to which individual perceptions did vary initially. These deviations vary slightly from curve to curve but are in general, of the order of 1.5 units [see Appendix 7 for details].

2. Research with social, economic and political potential was considered to be useful for

i. the further legitimation of the discipline of Physics;

ii. the further legitimation of the local School of Physics;

iii. the raising of funds for further research;

iv. attracting students to combat shrinking enrolments.

3. A gap in the research capacities of the local School was perceived.

4. Goals which could be shared by other researchers in the university might provide a basis for increased solidarity between relevant Departments and Schools. This sharing could provide a basis for

i. collaborative research programs for established scientists;

ii. the generation of Ph.D projects.

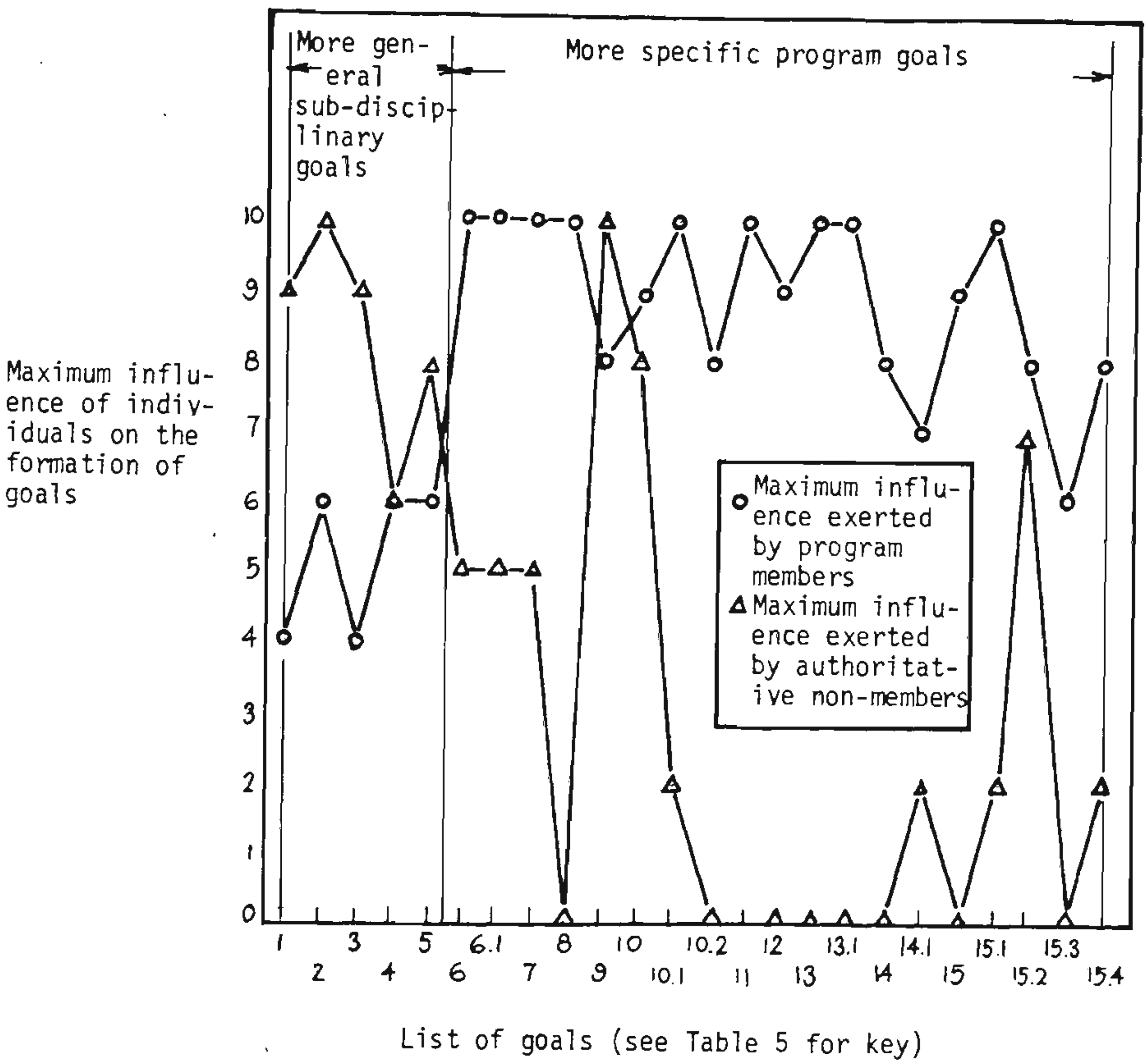
5. A successful program might provide economic returns to individual scientists and the School of Physics through royalties and consultancies.

In terms of the separation between contexts of research and legitimation the nature of the first five goals suggests that in the initial stages of the research program the separation may not have been so firmly entrenched in the minds of the researchers as it later became. Clearly though, by the time particular lines of research were well established these general goals began to fade in significance, and goals that were once pressing concerns became bracketed out along with the "popular science" of solar energy research. In this respect goals 1-5 were consensually agreed to have been almost fully achieved by April 1977, by which time the group was, for reasons of survival, very much concerned with producing rather more concrete justifications for their existence than broad claims about the strategic importance of their research. By September 1978, all but one (G1) of these five goals was felt to have been fully achieved as opposed to none of the other nineteen goals [see Figure 6.9-1].

It is noteworthy that the more general goals of the program tended to be imposed on the program members "from above". That is, as shown in Figure 6.9-2, the individuals who were most influential in the formation of goals 1-5 were authoritative figures who did not become involved in the day-to-day research of the program.[28] This situation helps to explain why these general goals were felt to be more readily achieved despite their rather indeterminate nature (compared with the more technical goals). In other words, these more general goals were more distant, and less part of a controllable world within the sphere of individual or collective autonomy.

This sense of personal and collective autonomy has been pursued through the calculation of an "autonomy index" for the individual scientists (that is, the numerical differences between individual scientist's levels of influence over the formation of goals and their levels of involvement in research with particular goals have been calculated - see Appendix 1). These indices have

FIGURE 6.9-2: Maximum influence of individuals on the goals of the SSP.



been averaged over the three most practically influential scientists (the "core group") in the program. These average figures are presented in Figure 6.9-4, along with the average priorities for this core group (the individual data on which this figure is based are tabled in Appendix 11). From this Figure it is apparent that the average autonomy index for the first five goals is lower than for the other goals, a result which lends some support to the notion that the actual sense of individual and collective autonomy over the more general goals was less than for the more technical goals. Nonetheless, as shown in Figure 6.9-4 goals 1-5 were considered to remain as relatively high priorities for the "core group" of three members throughout the period investigated. This must appear a little strange however, given the postulated high level of task orientation of the researchers. This apparent contradiction is somewhat clarified however if it is realised that the *assertion* of priorities by individuals need not reflect the reality of day-to-day involvement of researchers with particular goals. That is, the assertion of priorities is more likely to be an idealisation or, in other words, more likely to be in the context of legitimation rather than in the context of research as compared with the scientists' descriptions of their levels of involvement with goals in day-to-day research (and their descriptions of the levels of influence they had over the formation of the goals). For that reason the autonomy index is considered to be a more reliable indicator of the way the researchers actually confronted their research. In addition, the

FIGURE 6.9-3: Average priorities of the core group for particular goals of the SSP.

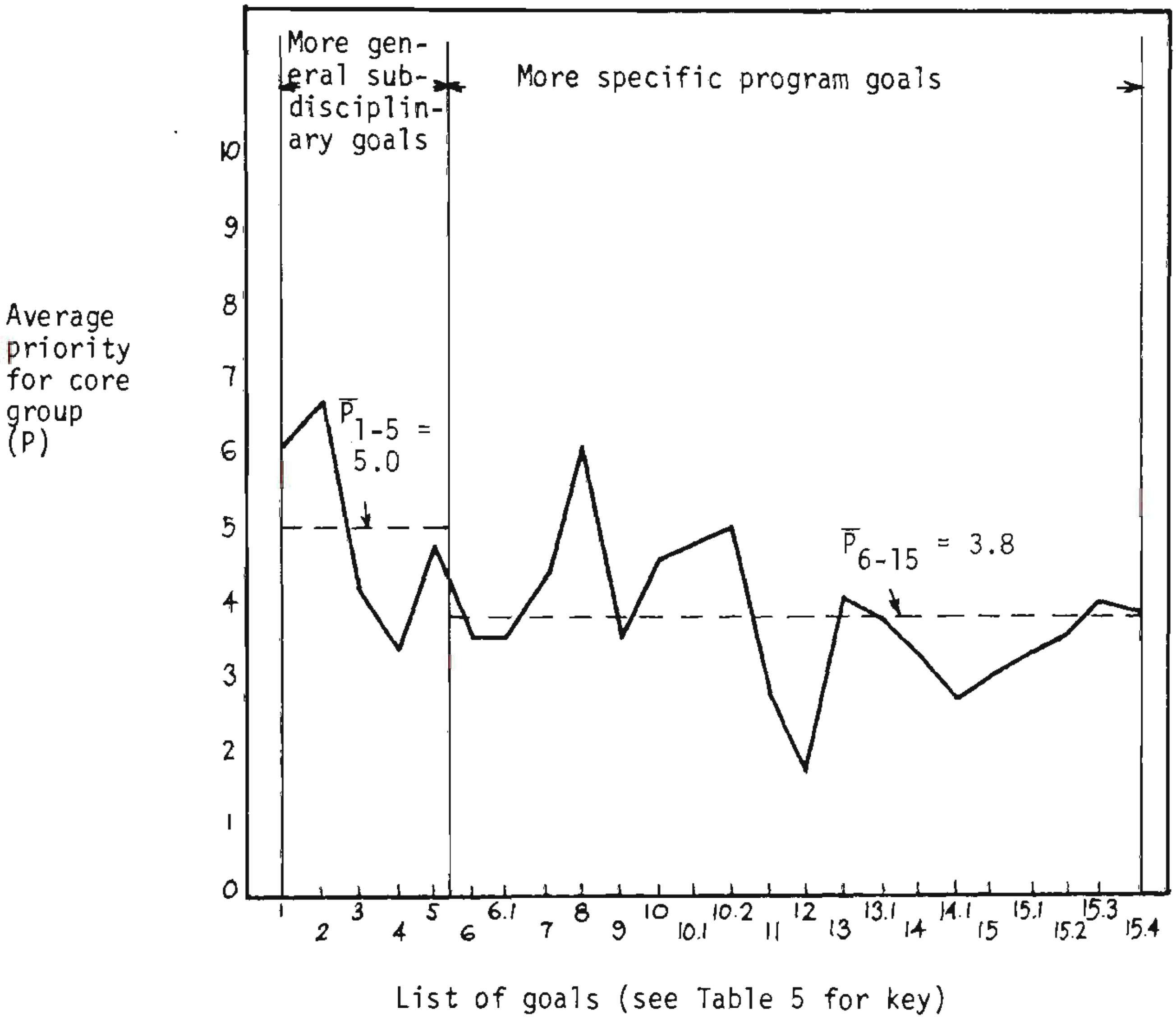
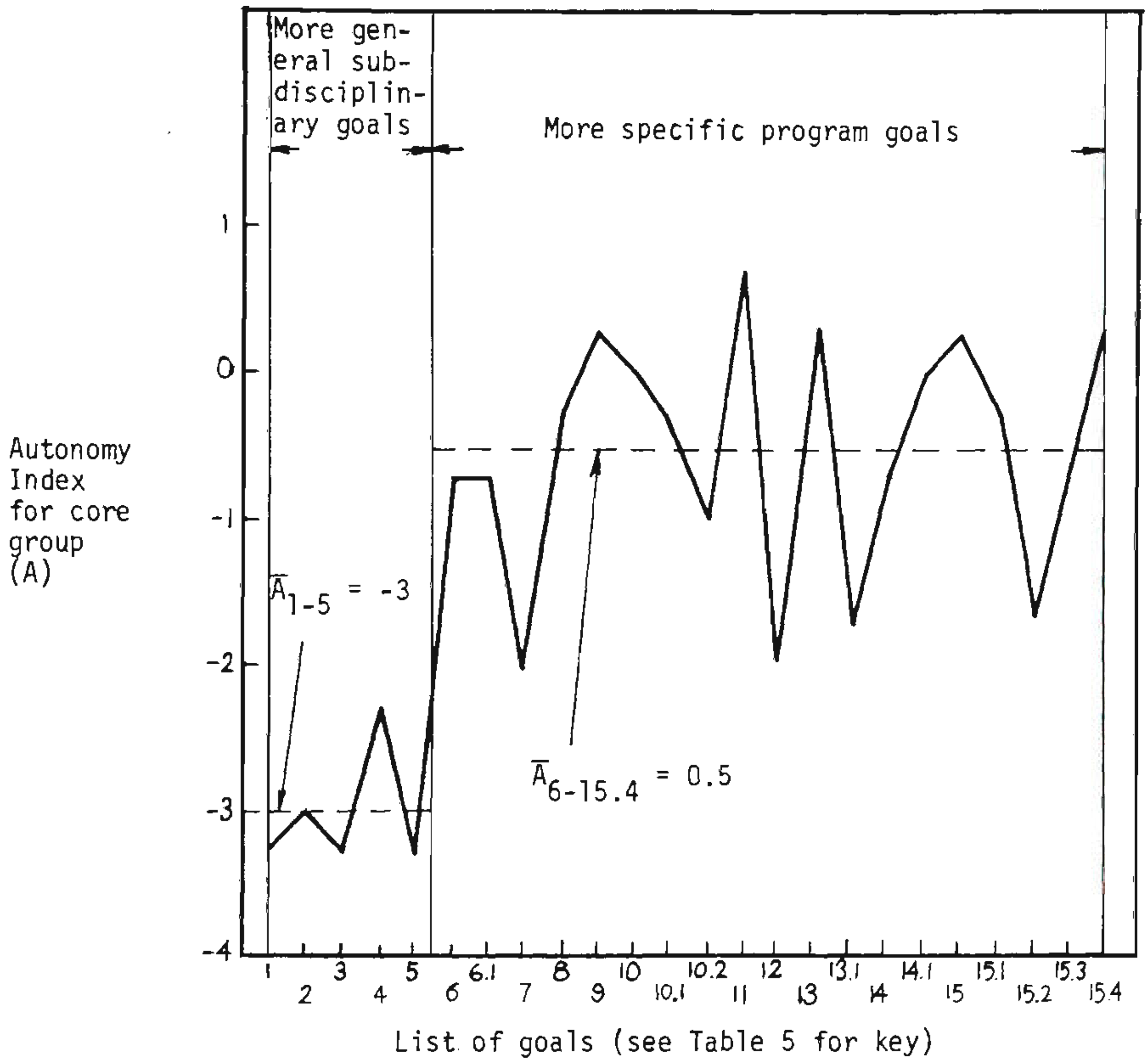


FIGURE 6.9-4: Autonomy indices for the core group of the SSP.



fact that the researchers did not consider those goals to which they attributed high priorities as being particular significant in accounting for the direction of their publications also supports our relegation of the asserted priorities more to the context of legitimation than research.

On a more general note however, it is taken for granted that the displaced goals are merely part of a range of goals and beliefs that together with the solar energy belief system defined earlier constitutes a *particular* context of legitimation for the researchers in this study.

(ii) The constellation of goals was not subject to sudden change as a consequence of the emergence of significant research events:

Most of the research goals (17 of the 24) were established before the occurrence of the two research events that were singled out in the last section as being instrumental in determining the nature of the final product (that is, events 9 and 28 in Figure 6.8-1). Considered in conjunction with the earlier remarks about the stability of the constellation of goals, this high level of organisation which remained relatively unchanged even following critical events in the evolution of the program, is taken as evidence of a high degree of cognitive institutionalisation of the researchers at the level of task orientation. As a further indicator of this high level of cognitive institutionalisation, serendipity was not reported as being of particular consequence to the overall structure of the research (this is in marked contrast with the case study that will be discussed in the next chapter). The only major serendipitous event was E28 in Figure 6.8-1, the discovery that an American firm had independently developed an evacuated glass collector. This event was in fact, only partially a "happy" accident (in that it offered validation of the concept, but also introduced a sense of competition) but in all events it was not of great significance to subsequent developments.

(iii) As the program of research evolved, the goals became increasingly technical in orientation, this being a natural consequence of the increasing concern of the group members with actually producing a working device (rather than just producing theory about devices which might work):

This tendency towards a technical orientation is reflected in the group's publication record.[29] Very few publications were felt by the scientists to be oriented towards the first five general goals - 11% of the group's 28 publications in 1976 and 1977 were thought to be directed towards one or other of goals 1, 2, 3 and 5. In comparison, all of the group's publications were felt to be directed towards one or other of goals 10, 10.1, 11, 12 and 13.

This technical orientation is not entirely explicable in terms of technical determination, however. For example, goals 7, and 15.4 were a partial consequence of the awareness that it would be prudent to avoid direct competition with state generated electricity. By and large, however, the social concerns of the more technically oriented goals appear to remain at a level of concern which revolved around the need to publish and the related need to nurture and protect potentially fruitful lines of research. In other words, the major focus of the group's publications was towards specific theoretical and technical issues.[30] Further details of the group's publication record are detailed in Appendices 8 and 9.

(iv) There was some degree of risk taking involved in appealing for major financial support for a project which was not fully proven from an engineering point of view:

Even though a successful selective surface and a likely candidate for the type of solar collector to be developed (that is, a tubular glass collector) were available, the problems of manufacturing a commercially viable collector system had only just been confronted (although the massive funding of the project was on the basis of a highly prospective commercial venture). Further, most of the group's technical problems had not been solved by April 1976, when a "major breakthrough" was announced in the university gazette. Nor had these goals been fully achieved by April 1977, although it was felt that considerable progress had been made (probably sufficient to warrant optimism about manufacturing a device for the commercial market). This issue will be dealt with further in the next section.

6.10 Communication problems

Summary: The differences between a context of research and a context of legitimation have been further examined by showing (i) that information about the SSP flowed out of a context of research (H4) through a *process of communicative distortion, and (ii) that this process of communication was involved with the needs of various people to legitimate a practice oriented research program with an uncertain future (H14).*

As discussed in the opening chapters of this thesis, a highly specialised, professionally based division of labour is based upon the separation and high level of institutionalisation of semi-autonomous "finite provinces" of meaning, and within these universes of meaning, the separation and high level of institutionalisation of semi-autonomous sub-universes of meaning. The problems of communication between specialist and lay person (and even between different types of specialist) that have resulted from this specialisation of expertise are notorious and hardly need to be established here as important problems. The purpose of this concluding section is rather to examine in a little more detail a particular instance of distorted communication about the selective surfaces program. In this section I have attempted to provide further justification for the claim that the contexts of research and legitimation were separate sub-universes by showing, (i) that information flows out of a context of research where it becomes subject to mediation by interests other than the scientists primarily responsible for generating the information,or in other words, where it becomes subject to distortion; and (ii) that the processes of communication dealt with here are caught up with the needs of a number of people to legitimate a research program with an uncertain future. Some evidence demonstrating the exaggeration of specific claims has been advanced to further support this contention.

To avoid any possible misunderstanding I would stress at the outset that problems of distorted communication are not necessarily created deliberately, but more often than not arise from social situations with dynamics that transcend individual intentions. Specifically, what follows should not be construed as an attack on the moral integrity of the people involved. Rather, the analysis which follows should be appreciated as dealing with an example of a typical situation where communication between specialist and lay person is associated with various types of distortion, in this case the selection and exaggeration occurred when information was transmitted via various media, beyond the boundaries of a sub-universe of meaning (in this case, the context of research).

Table 6.10-1 is a schematisation of the media based communication surrounding the eventual heavy funding of the selective surfaces program. A critical point in the funding process was an announcement made by the group in April 1976, in a newspaper published by the university administration. This announcement concerned the "breakthrough" that the group had made in developing their selective surface and the associated "great strides" that had been made towards the development of high temperature solar collectors. The announcement clearly reflected a decision that had been reached about the level of development of the program and the commercial viability of its main products. This decision was, no doubt, prompted by the awareness that funding was running out and that the group's survival was in jeopardy - which was the substance of a press release (in the same month) that had been

TABLE 6.10-1: The successful story of a research group in search of support.

	Selected events:
April, '76	Press release by group: funding was urgently needed because the group "despite substantial progress, may disintegrate in the New Year, a victim of the impending cuts imposed on the University by the Federal Government".
April, '76	Announcement in the local University Newsletter: "Breakthrough in solar energy research".
June, '76	The Australian Information Service circulated a story which was scooped (three months later) by Daily Kabul Times (Afghanistan): "A Hot Line to the Sun". The story was practically ignored by the local press, but gained coverage overseas in at least nine different countries.
February, '77	The Head of the Physics School announce the Australian "breakthrough" to the press in London. Only then did the Australian press seize on the newsworthiness of the local events.
April, '77	The local state government to the rescue: $1.08 million in funding.
November, '77	The Arabs follow suit: $5 million over three years from the Saudi Arabian government.

apparently ignored by the media. Thus, it is probably more accurate to say that a decision had been made that the program's assets were sufficient to risk the public scrutiny that might follow media coverage and a very generally directed appeal for funding. The "assets" of the program at this time were in fact quite strong. By all indications (including the "corrected" levels of achievement of the key goals listed in Table 6.10-3 which are dealt with shortly) the group had reached a stage in pursuit of the goal of a commercially viable collector which employed the group's "own" selective surface, where thoughts could be seriously turned towards the problems of entering the solar collector market. The successes of the group were eventually taken up by the media, but only after the local press had been "scooped" by the Daily Kabul Times and subsequently given the lead by the Daily Express in London.

The story that eventually reached the Australian public through the media was, naturally, a very partial account of the activities, beliefs and successes of the group. Tables 6.10-2 and 6.10-3 are attempts to show the extent of the filtering of information involved over the ten month period between the first press release by the group and the eventual coverage by the Australian media. Both of the Tables show the extent of the selective communication of particular components of the Solar Energy Belief System and the goals of the selective surfaces program. The indications are that only a very few of the beliefs and goals of the researchers on the selective surfaces program were ever communicated outside of the context of research. All of the stories listed were "careful" in the way that they expressed the

TABLE 6.10-2: The selective communication of components of the Solar Energy Belief System.

Particular Communications	Components of the Solar Energy Belief System that were communicated out of the context of research*											
	1	2	3	4	5	6	7	8	9	10	11	12
St. Louis Dispatch (3/77)					5	6		8			11	
Press Release (4/76)	1	2			5	6		8			11	
University Newspaper (4/76)						6					11	12
Daily Kabul Times (9/76)					5	6		8			11	
Daily Express (2/77)					5	6		8				
The Sun (2/77)					5	6		8			11	
The Age (2/77)					5	6		8				12

Key to components [from Table 6.6-1]:

1. Solar energy can, in the long term, be a significant contributor to global energy supplies (i.e. in more than 10 years).

2. Solar energy has most potential as part of a "pluralist" energy system (including nuclear as well as fossil fuel based systems).

5. Solar energy could make a significant impact on most natural energy supplies in the short term (i.e. within the next ten years).

6. Solar energy is important as a potential source of low grade heat (i.e. at temperatures less than 300°C).

8. Solar energy can uniquely provide valuable specialist application.

TABLE 6.10-2 (cont.)

Key to components (cont.)

11. The main knowledge deficits are not scientific.

12. The main knowledge deficits are technological.

TABLE 6.10-3: Filtered expressions of the goals of the selective surfaces program

Particular communications	Particular goals that were communicated out of the context of research. Note: In terms of goals, this context comprised goals 1-15.4, a total of 24 goals										
Press Release (4/76)		5	6		7	8	13.1		15		15.3
University News-paper (4/76)	4	5	6	6.1	7	8	13.1		15	15.2	15.3
Daily Kabul Times (9/76)	4	5	6		7	8	13.1		15	15.2	
Daily Express (2/77)		5	6		7	8		14	15		
The Sun (2/77)		5	6		7	8		14	15		
The Age (2/77)		5			7	8			15		
St. Louis Dispatch (3/77)					7				15		

Key to components [from Table 6.6-1]:

4. Develop an alternative to flat plate collectors.

5. Incorporate selective surface research with solar energy research.

6. Find a new, efficient selective surface.

6.1 Develop a refractory selective surface.

7. Develop a system which produces heat in the intermediate range of 100-300°C.

8. Develop a commercially viable collector which employs the new surface.

13.1 Develop the sputtering technique.

TABLE 6.10-3 (cont.)

Key to components (cont.)

14. Develop an evacuated, tubular sputtered glass collector.

15. Develop commercial applications for a working solar array.

15.2 Develop a configuration of collectors and concentrating mirrors.

15.3 Check degradation mechanisms of the materials involved.

commercial potential of the program, and avoided making inflammatory claims about the importance of solar energy as an energy source. It is also apparent from the Tables that different audiences have access to different stories - in particular, the University newspaper story is markedly less expressive of the beliefs of the researchers than are the other communications listed. As one moves out of the local academic context the story becomes much more "newsy" and there is a further loss of technical details and an increase in the ratio of beliefs to goals expressed in the stories.

As well as the occurrence of a process of information filtering it is also significant that there is evidence for some exaggeration of the achievements of the group. That is, no doubt, typical of optimistic communication about the potential of any research, but at the same time as being evidence for natural exuberance it is also quite clearly evidence for the willingness of researchers to take risks, particularly if the stakes are high. In this case it was a question of the continued viability of a research group, and although there do not appear to have been any outrageous claims made, the evidence presented in Table 6.10-4 suggests that the level of achievement of the goals that was implied in the university newspaper article was, for six of the goals, higher than the levels of achievement reported to me a year later. On the basis of the average reported rate that the scientists indicated about progress towards the goals over the three year period under consideration, it is possible to "correct" the levels of achievement reported by the scientists in April 1977 back to April 1976 levels. These corrected levels reveal a consistently modest

TABLE 6.10-4: Exaggerated claims about the levels of achievement of the goals of the SSP.

Goal	Level of achievement implicit in the announcement in the University Newsletter (April, 1976)	Level of achievement reported at April 1977	Corrected level (April 1976)
4	H	H	M-H
5	H*	H*	M-H
6	H*	H	M-H
6.1	H	H	M-H
7	M-H	H	M-H
8	L-M	M	L-M
13	H	M-H	M
13.1	H	H	M
14	H*	H	M-H
14.1	H	H	M
15	L-M	L	L
15.2	M-H	M-H	L-M
15.3	H	M-H	M

Key:

	Level of achievement	Range on a six point scale**
H:	High	4-5 inclusive
M-H:	High to medium	3-4
M:	Medium	2-3 inclusive
L-M:	Low to medium	1-2
L:	Low	0-1 inclusive

* Fully achieved goals

** Note that a more precise presentation of the reported levels is contained in Figure 6.9-1.

level of exaggeration in eleven of the thirteen goals implicated in the "breakthrough" story.

6.11 Conclusions

On the basis of the empirical material presented in this chapter we have seen how a group of scientists performed research as part of a research program which was constituted through the collective activities of a group of research workers who shared a commitment to particular research practices and techniques, who were directed in their research towards a shared set of goals and who shared, to some extent, a common stock of specialised knowledge (H12).

The broad goal of the research presented in this chapter was to provide an analysis of the institutionalisation of a solar energy research program. Efforts were made to describe empirically the cognitive field of a group of research workers and to appreciate the various structures and processes involved in the constitution of this field as being highly social in nature. One of the major themes that has been pursued in this chapter is the idea that science is a goal directed process. In this chapter it has been demonstrated how scientists are not only directed towards goals which are highly theoretical and technical in nature but also that scientists are often highly influenced by goals that are value laden and part of the social, economic and political dimensions of research processes that are often presented by scientists as value free.

Towards this end efforts were made -

(i) to identify the various goals that strongly influenced research on the Selective Surfaces Program;

(ii) to locate the goals as part of a structured cognitive field which was particularly influenced by "professional" reference groups; and

(iii) to relate the goals to a process of research.

As a result of these efforts, strong support was provided for three hypotheses that were centrally concerned with the structure of scientific research. Research on the SSP was demonstrated to occur in the context of a structured cognitive field which consisted of a number of different levels (metaphysical, theoretical, subject concerns and technical levels [H6]). A theoretical landscape and a constellation of goals were two particular cognitive structures which were shown to provide a context of relevance for research on the SSP (H7). This constellation of goals was an evolving structure of goals which covered a range of interests and which spanned different levels of the research program (H8).

Attention was also directed to the processes involved in the production and reproduction of these structures. For example, the goals of the members of the SSP did change over time, as was expected (H10), but this change occurred as a process of sedimentation of new goals into established structures rather than by the replacement of old goals.

In broad summary, the goals of research were shown to be a product of social as well as cognitive factors, an exercise which is of primary importance to any thorough investigation of the relative extent to which different research processes are constrained by social, economic and political factors, such as will be attempted in Chapter 8.

In the case study I have attempted however, to move beyond the "mere" establishment of a phenomenologically informed framework for the generation of "research accounts". It was felt necessary to capture something more of the spirit of modern science than the observation that the natural sciences are highly subject to the authority of cognitive structures. It was suggested that the research process was in fact, subject to, and in various ways problematic because of a variety of conflicts, stratifications and separations. These findings lend support to many of the hypotheses listed in Chapter 5 and can be conveniently summarised in a similar form. Of these hypotheses only those which involved a comparison between research programs have not been discussed in some detail. Those hypotheses (13, 14, 15, 16, 17, 18) will be dealt with in Chapter 8.

Researchers in the SSP were subject to the social and cognitive control of professionalism which operated through the agency of the professional orientational reference groups of science and engineering (H1). These reference groups provided a basis for the scientists' distinctions between, and definitions of scientific and non-scientific activity (H2).

The scientists appeared to operate in, and move between two different contexts within the sub-universe of the research program - a context of legitimation and a context of research (H4). This movement between contexts (themselves sub-universes) was associated with a conflict of relevancies. This conflict was observed as a "double bind" situation where scientists experienced the, at times, conflicting demands to be both professionally competent and socially useful (H5).

Researchers in the SSP tended to bracket social considerations about their research as "external" to the research process. As a consequence of this, the more social aspects of their research were perceived by the scientists as part of a Solar Energy Belief System (H3). This belief system is a body of ideas about solar energy which is shared by most solar energy researchers and formed the major component of the metaphysical level of the cognitive field of the members of the SSP (H6), but because of its more social orientation did not provide significant relevancy in the context of research (H7).

The structure of goals that was shared by program members was stratified between more socially and more technically concerned goals: in general, the more socially oriented goals were felt by the researchers to have been successfully achieved quite early in the program's history - effectively leaving the scientists "free" to get along with the more problematic technical aspects of the program. That is, the research of the members of the SSP became increasingly technical in orientation as the program evolved (H11).

Not all of the goals of research of the members of the SSP remained equally relevant over time. Those more general goals which were partially established in the context of legitimation, and effectively imposed from above, were subject to displacement from the context of research to the context of legitimation (H9 and H11) wherein their relevance to day-to-day research was diminished (H3).

The differences between a context of research and a context of legitimation were further examined by showing, (i) that information about the SSP flowed out of a context of research through a process of communication distortion, and (ii) that this process of communication was involved with needs of various individuals to legitimate a practice oriented research program with an uncertain future.

In this chapter it has been demonstrated that despite a fairly low level of institutionalisation at a disciplinary and specialty level (which was examined in terms of the "marginality" of solar energy research) research in the SSP was highly institutionalised - thus, for example, once a particular goal had evolved and received a measure of commitment from the researchers, it did not change over a period of time as long as, in some cases, four years.

The material presented in this case study also clearly points up the considerable distance that might be reasonably expected to separate any academically oriented processes of research and the "in house" development of the products of that research - after all, the separation of the context of research from a context of legitimation is comprehensible both in terms of the separation of the finite sub-universe of meaning of a research program from a broader "world of science" and also in terms of the separation of research from the "paramount reality" of everyday life.

A more detailed historical analysis of the origins of the conflicts that have arisen as a result of this separation would require another thesis, but there is one important observation that can be made in this conclusion without risking too much controversy - the tension in physics between research and the social justifications of research has demonstrated considerable intensification since World War II. That is, it is quite clear that since World War II ("the physicists' war") and the advent of the nuclear bomb, the discipline of physics has, metaphorically speaking, begun to show fairly obvious signs of a guilty conscience. That is to say, even though most physicists (and natural scientists in general) still tend to preserve fundamental distinction between theorising about something and action in the world, the continual recurrence of debates involving physicists in issues concerning the "application" of physics (and science generally) in publicly sensitive areas indicates that there is at least some awareness on the part of physicists that there is a problem involved in the relationship between the world of physics and the "outside world" where knowledge is "applied". The involvement of physicists in the development of a nuclear industry and the associated proliferation of nuclear power stations and nuclear weapons, their involvement in warfare through weapons researcn and their involvement in enormous extravagancies such as the space race, are the most obvious examples of the kinds of issues that have prompted something of a "legitimation crisis" in physics.

FOOTNOTES TO CHAPTER 6

1. The kind of research being dealt within this thesis is predominantly "academic" in general orientation. It should not be assumed however, that the "experiments" of those attempting to practically incorporate solar energy as part of that life style are not worthy of the title "research". The important difference for the purposes of this thesis is that I am dealing here with research that consciously orients itself to more traditional, highly institutionalised, predominantly academic, scientific reference groups.

2. By contrast, if Australian R & D expenditure is taken as a whole, approximately 38% of expenditure in 1973-74 occurred in the "Business" sector. [See Project SCORE - Research and Development in Australia 1973-74, Canberra: Department of Science, 1976].

3. This figure is based on estimates given to me by researchers in my interview sample. According to the Australian Department of National Development, solar related projects in the year 1976-77 received $3.1 million in funding, or 11.6% of the total Australian energy R & D budget [Department of National Development, Energy Research and Development in Australia 1976-77: A National Survey, Canberra: AGPS, 1978].

4. For example, one recent estimate put solar funding in the U.S. at less than one five-hundredth of federal energy funding and this is despite an increased general awareness of the potential of solar energy as an energy source. Figure quoted in Helen Drusine "Solar Politics", Omni (January, 1979). Estimates vary from source to source but the upper limit for advanced industrial economies appears to be something of the order of two to five percent of publicly funded energy budgets. In 1973 the leading capitalist economies spent between 60-80% of their publicly funded energy research budgets on nuclear fission [Energy Policy, June 1975, pp.90-115].

5. This is the consensus of opinion amongst my sample of Australian solar energy researchers. There are, unfortunately, no mass survey statistics readily available to support these observations.

6. Again based upon a consensus of opinion amongst my sample of solar energy researchers.

7. For example, the three most senior members of the selective surfaces program had done their Ph.D work in different areas of solid state physics before coming to solar energy research [see the Key to Figure 1 for further details of the development of the research interests of the members of the selective surfaces program].

8. That is, there are certain specialist applications of solar devices which could not at present be economically replaced by systems based on the consumption of non-renewable resources, for example, the use of photovoltaic cells for the generation of power in isolated areas such as satellites, the cathodic protection of pipelines, and in navigation aids such as illuminated ocean buoys.

9. In the history of science one need only reflect on the classic cases of Galileo and Darwin. Marxism provides another more contemporary illustration of particular relevance to the social sciences.

10. Solar Energy, the journal of ISES, the International Solar Energy Society.

11. In Jagtenberg [1975] and Johnston and Jagtenberg [1978].

12. Although there were relatively large numbers of people involved in the range of professional relationships that are typical of any research effort (the ones investigated were co-worker, regular colleague, co-author, supervisor, student, technical assistant, occasional colleague, communication through the literature, elite peer, and representative of funding organisation) colleagues, co-authors and students were mostly confined within a local university environment. Seventeen of the twenty-two colleague, co-author, and student relationships reported to me were confined to the "local" environment.

13. See the footnote to the key to Figure 6.8-1.

14. Although it should not be implied that scientists are normally quite replacable (that is, expendible) for as Collins [1974 and 1975] demonstrates, there are some situations where the art of making something work is not easily transferrable from person to person.

15. For example, the British periodical Undercurrents, and the Australian periodicals Simply Living and Earth Gardener.

16. As mentioned in Section 5.4-1 I have had discussions with people devoted to "alternative" life styles in England and Australia - the most memorable having been in rural Suffolk and Wales, Manchester, Bath and the "Rainbow Region" of the north coast of Australia. Whether in England or Australia the self imposed definition of an "alternative" life style usually signifies a practical as well as ideological break with many of the standard practices of everyday life in an advanced industrial capitalist society. Typical targets are health, and medicine, diet, and clothing - a more detailed account is beyond the scope of a footnote.

17. Rose and Rose [1976b:14] write of "incorporation" in a slightly different sense as markin a change in the mode of production of science: between the nineteenth century and 1945 science has changed from an essentially craft based activity to an industrialised process. This change has emphasised science's twin nature as a force of production and of social control.

18. As testified by what historical accounts there are - a fairly slim collection, in fact. See, for example, Farrington Daniels Direct Use of the Sun's Energy, New York, N.Y: Ballantine, 1964; D.S. Helacy Jr., The Coming Age of Solar Energy, New York, N.Y: Avon, 1963; and Behrman, Daniel, Sola-Energy: The Awakening Science, Boston, Mass: Little, Brown and Co., 1976. See also the major sources listed under Table 2.

19. This is quite apparent in the primary sources that were used, as a basis for the construction of the solar energy belief system. It is very difficult to accurately locate this displacement in time (and perhaps it is not able to be uniquely located), but it would appear that this displacement was in evidence as early as 1953.

20. For example, on the basis of the sources of Table 1, and a now burgeoning literature on alternative energy strategies - D. Halecy's The Coming Age of Solar Energy, New York: Avon, 1973; W. Patterson's Nuclear Power, Harmondsworth: Penguin, 1976; and A. Lovin's Soft Energy Paths, Harmondsworth: Penguin, 1977.

21. This observation of task orientation is by no means new but it is more unusually quoted as typifying an industrial or "applied" context rather than a primarily academic situation. See for example, Hill [1974] and Cotgrove and Box [1970].

22. This was tested by asking two of the program members to locate the goals in Table 6.7-2 in the theoretical landscape in Table 6.7-1. It was quite clear from the exercise that the theoretical implications of the goals were different for the two members despite their agreement about the items in both the Tables. The level of agreement between the two researchers was in fact surprisingly low; of the one hundred and seven different locations of goals in the theoretical landscape that were listed between the two scientists they only agreed seventeen times [see Appendix 10].

    The main implication of such a low level of agreement would appear to be that even within a highly institutionalised environment the task of interpretation will be initially approached by scientists from an egoistic perspective, that is, how are particular goals related to personal needs and activities. This does not mean that individual researchers cannot take the attitude of "the other" nor form an overall perspective of the group's research. These more "meta" perspectives were not explored in the particular exercise under consideration.

23. It is interesting to note that when the goals are arranged chronologically they still tend to demonstrate an ordering which becomes more concrete as one proceeds down the list. That is, specific research tasks tended to evolve on the basis of previously established goals of a higher level of generality, (but only of course on a "co-present" basis of prior research that is, the process is probably more dialectical than linear).

24. These social factors fell into two inter-related categories:

    i. "General" political/economic strategies, aimed at, for example, the further legitimation of the discipline of Physics, the raising of funds, combating declining student numbers, etc.

    ii. Considerations of career advancement and personal advancement such as the production of publications, patents, royalties, and patents.

    See also Table 6.9-1: The original list of social factors is contained in Appendix 3.

25. In spite of this "blinkering" it is still apparent in the interview material that political/economic strategies were nonetheless important in the early stages of the program's development when goals of a more general nature were being formed and became less important as directions to research (from the scientists' point of view) as their research became more technically oriented.

26. Only one researcher indicated that his first priority did actually fluctuate depending on what he was into at the time. Apparently the rest of the researchers regarded any changes of interest as minor aberrations in the face of the authority of their established priorities.

27. Researchers were asked, in a questionnaire, to estimate progress towards the goals using six point scales (0-5). The figures presented are the averages of the individual responses.

28. Researchers were asked, in a questionnaire, to estimate the influence of significant individuals in the formation of goals using six point scales (0-5). The figures presented are the averages of the responses. Both of the professors of the School of Physics were protagonists in the category of authoritative non-members, but one was far more influential than the other.

29. It should be added that publication was considered to be very important by all the researchers in the SSP. This is apparently typical of most university based scientists - see for example, Cotgrove and Box [1970].

30. Although it should be pointed out that some of the research conducted was of a *highly* theoretical nature (for example, some of the work on "cermets" - see Figure 1). In other words, not all the research was practice oriented and directly geared in to the production of commercially viable products.

CHAPTER 7: CASE STUDY 2. ASPECTS OF THE INSTITUTIONALISATION OF AN AUSTRALIAN NEUROPHARMACOLOGICAL RESEARCH PROGRAM

> It has recently been established, by chemical analysis, that several species of bean contain large amounts of L-DOPA (of the order of 25 gm. L-DOPA in a pound of beans). There is also a suggestion (which requires careful examination) that such L-DOPA rich beans may have constituted a 'folk-remedy' for Parkinsonians for many centuries, if not longer. Thus although we ascribe 'The Coming of L-DOPA' to A.D. 1967, it may well have 'come' by 1967 B.C.
>
> Oliver Sacks, Awakenings, Harmondsworth: Penguin, 1976, p.47.

7.1 The purpose of the case study

The case study which follows is a self contained analysis of similar empirical depth to the solar energy case study presented in the last chapter but the focus will be more directed towards comparing the two research programs. This approach reflects the fact that in sociology there are very few (if any) judgements of an absolute nature that can be made. Thus, to speak of levels of institutionalisation and levels of orientation towards social application in a particular empirical context entails an implicit comparison with some other part of social reality. The precise articulation of a basis for comparison is not always easy, but the exercise is a worthwhile one, even if clarification of an argument is the only achievement. In this chapter the same theoretical and methodological guidelines have been used as in the last chapter and so the following case study is a continuation of the attempt to ground the theoretical and methodological analysis that has been developed so far.

The comparative basis of this chapter has been partially oriented towards those hypotheses dealing with levels of social constraint, practice orientation and institutionalisation of contexts of legitimation, but a systematic comparison of the two programs will not be undertaken until the next and final chapter. Whilst attempts have been made to present the following case study in the same form as the last case study (to facilitate comparisons), this strategy has not been pursued at the expense of data and insights which move beyond or, in places, not as far, or in slightly different directions to the last case study. This approach is fully consistent with the generally dialectic approach that has been adopted in this thesis.

7.2 Introduction and brief summary

The DOP was, in many ways, similar to the SSP. It was a university based program of a small to medium group size of five scientist members (as opposed to eight scientist members in the SSP), and was a relatively new program having been established only one year at the time I began my research (as opposed to the two years that the SSP had been established at the equivalent time). The time span of my research account of the DOP was smaller however (two years as opposed to three and a half years for the SSP). There were other significant differences as well-the programs had different disciplinary bases, different levels of practice orientation, different levels of institutionalisation (including different levels of funding) and different legitimation needs. These differences will be explored at some length in this chapter.

The neuropharmacological research program under consideration in this chapter was directed towards the general goal of the elucidation of dopaminergic and octopaminergic mechanisms in the human brain, and their role in schizophrenia. More specifically, the program had two major complementary theoretical goals:

i. the elucidation of the biochemistry of a series of dopamine related chemical compounds that are involved in chemical transmission systems in the brain; and

ii. the elucidation of the role of dopamine and octopamine in the biochemical mechanisms associated with schizophrenia. [1]

Research on the DOP was defined by the scientists as "basic research", however this definition should not obscure the fact that the goals of research were the partial products of scientific and medical professionalism, and that furthermore, the scientific and medical professions mediated all scientific action. One of the major long term goals of the program was the production of a drug for the cure of schizophrenia and this is taken as further evidence in support of the hypothesis that most scientific research should be considered as directed towards goals that may be highly socially mediated.

Once again I would stress that although goals of research have been treated as part of the *cognitive* institutionalisation of the research program they make very little sense in isolation from the social institutionalisation of the program.

In other words, the DOP did not exist in a social vacuum. The DOP was however, not highly institutionalised - the fact that the discipline of clinical pharmacology has been used to describe the program's overall location is more a product of a fairly recent consensus by specialists that such a discipline actually exists than the description of a clearly defined and demarcable set of cognitive interests. The same remarks apply equally to the description of the program as situated within the specialty of neuropharmacology. In this case it appears that the specialty neuropharmacology is not particularly distinct from other related specialties that have been identified from the literature, for example, psychopharmacology or neurophysiology. On the basis of my interaction with the workers on the program it appeared that it was less important for them to be associated with a specialty (the abovementioned specialties were rarely referred to in conversation) than to be identified with a broader disciplinary perspective (that is, clinical pharmacology). This apparent failure to identify with a specialty may be due to as yet incoherently defined specialties, or perhaps even to mislabelling in that clinical pharmacology is a specialty, synthesising aspects of medicine and biochemistry. It does however, seem more likely that these researchers, like the solar energy researchers discussed in the last chapter, were predominantly "task oriented". That is, the scientists involved with this program were not particularly concerned with the (for them) largely philosophical problem of deciding their precise location within disciplinary structures of relevance - such a problem had little apparent relevance to the research tasks at hand. And so, once again it has been sensible and expedient for the sake of practical research to concentrate more on the most concrete unit of social organisation available: the research program.

This is not to say that more abstract concepts such as "specialty" and "discipline" are unimportant in understanding the DOP - in fact, some of the burden of this chapter has been showing how an incoherence of definition of "mental illness" and "schizophrenia" across specialties and disciplines is *at least* sociologically important in that this is evidence in support of the judgment of a relatively "low level" of institutionalisation of the cognitive environment of this research program. However, it should be apparent by now that the mere identification of a parent specialty or discipline is not adequate in accounting for the particular goals and directions of a research program - an analysis of the social processes of goal formation is at least required to provide a foundation for an in depth analysis.

Compared with the SSP, the DOP was not as highly institutionalised, cognitively or socially. Thus, for example, the level of serendipity that was incorporated into research in the DOP was considerably higher than that in the SSP - this indicated that choices had to a lesser extent been foreclosed by the authority of pre-formed social and cognitive structures. Furthermore, the establishment of the research program followed a path more of gradual evolution incorporating the resources immediately available than the simple adoption of pre-formed research strategies. In addition to this lower level of cognitive institutionalisation, the DOP was constituted in a complex and unstable economic support system. Overall then, the DOP can be shown to differ significantly along a number of dimensions when it is contrasted with the SSP. A number of preliminary comparisons will be made in this chapter, but a more comprehensive comparison will be reserved, as mentioned, for the final chapter.

7.3 Another struggle for survival

Summary: *Whilst also being engaged in a struggle for economic survival, researchers in the DOP were in a somewhat different position to researchers in the SSP. The double bind of professional competency versus social utility was far more distant for the DOP researchers who were engaged in basic research which did not, from the perspective of the scientists, particularly require social justification to enhance its survival prospects (H14). This basic orientation contrasts with the greater practice orientation of the members of the SSP (H13). The members of the DOP were subject to the social and cognitive control of both a scientific and medical professional orientational reference group (H1), but research in the program was dominated by a scientific orientation which excluded more practice oriented medical research and therapy (H2). One consequence of this relatively low level of social concern was that conflict deriving from alternation between a context of research and a context of legitimation was less noticeable in the attitudes of scientists (H5).*

Compared with the SSP, the DOP demonstrated a relatively low level of institutionalisation. This was indicated by a very diverse

support structure and the recent appearance of the discipline of Clinical Pharmacology in Australian universities.

The Dopamine/Octopamine Program (DOP) was, in the period of study, part of a complex organisational structure. This complexity did not, however, entail a high level of economic security for the research program. Quite the contrary in fact: the DOP was initially established on a limited contractual basis with a variety of sources of support. Most research is, of course, conducted within some time horizons, but usually there are some underlying economic structures of a relatively stable nature (such as a university school or department) which at least provides an overall context for research, if not some security of tenure for research workers working within that context (as was the case for the SSP). The DOP did not however have even that underlying basis of support: the Department of Clinical Pharmacology within which the research occurred was only minimally supported by the university - being housed in a hospital way away from the university and being largely financed by a drug company. The DOP was largely staffed and funded by virtue of drug company and National Health and Medical Research Council (NHMRC) support, the university providing only limited support in the form of some salaried workers and overall professional legitimacy. Further details of this support structure will be provided shortly as contributing evidence for a relatively low level of social institutionalisation when compared with the Selective Surfaces Program (SSP) which was discussed at length in the last chapter. First though, some appreciation of the broader institutional context of pharmaceutical and clinical pharmacological research will provide a firmer basis for the more narrowly focussed parts of the analysis which follows.

The discipline of clinical pharmacology has only recently become institutionalised in universities in Australia, although it has been established overseas for some time - this is in marked contrast to the disciplinary bases of the SSP (Physics and Mechanical Engineering) which have been long established in Australian universities. The particular Department dealt with in this case study was established in 1970 largely through the efforts of the leader of the DOP and was the first department of clinical pharmacology in Australia. Although clinical pharmacology is sufficiently socially and theoretically defined to be called a discipline (with the relative autonomy that the term implies), there is still a very close relationship between clinical pharmacology and the pharmaceutical industry - the industry being a major user of the products, processes and skills which have resulted from the establishment of clinical pharmacology as a discipline. For example, one of the general functions of the discipline is the evaluation of medically prescribed drugs. This close nexus with the pharmaceutical industry has probably been responsible both for the possibility of the discipline and the hesitation that universities have had in fully embracing the discipline as part of the academic environment.

This nexus exists despite the fact that in the discipline there may be at times little strong obligation experienced by researchers to perform highly practice oriented, medically relevant research. Thus, in the case of the DOP research was relatively unconstrained by demands of relevance from drug company funders - the DOP was in fact a program of relatively basic research (compared with the SSP, for example). Nonetheless, the presence of the pharmaceutical industry was pervasive and although researchers enjoyed the apparently easy-going largesse of one particular drug company they were still (quite happily) aware that the general institutional context of their research was permeated with the interests of the pharmaceutical industry. This was reflected in the long term goals of the DOP - for example, "to produce a drug to alleviate the symptoms of 'schizophrenia'", "to develop processes of screening drugs", etc. In comparison with the SSP, however, the drug company funding was less overtly contractual than the heavy funding that the SSP eventually received from the NSW State Government and the Saudi Arabian Government. And, in fact, because the needs of the drug company were a much more familiar part of the overall context of research, and could be accommodated in ways that still permitted the relative autonomy of basic research, there was less motivation in the DOP to achieve practically useful results.

There are a number of reasons why the pharmaceutical industry does support university conducted basic research in Australia at what seems to be the expense of more immediately useful practice oriented research. Firstly, the industry is strongly science based and has an established tradition of large research spending.[2] This research orientation reflects both the fact that the industry is strongly science based and that it needs novelties as well as established drugs.[3] Secondly, in Australia the pharmaceutical industry is too small to warrant the establishment of a wide range of company based research facilities.[4] It is considered more sensible to contract research out to competent and willing researchers in universities, or even more sensible still, to share in the establishment of research facilities such as occurred with the establishment of clinical pharmacology as a discipline in universities (note that this tendency is a general one and not restricted to Australia). Thus, according to the Australian Pharmaceutical Manufacturers' Association,

> "A survey of 29 pharmaceutical companies revealed that their current expenditure in Australia on research and development rose from $2.3 million in 1971 to $3.6 million in 1973. It is estimated that the amount spent in Australia during 1975 on company-financed research and development was about $4.5 million . . . Research expenditure in 1973 was budgeted on the basis of medical research 30%, fundamental research 11%, new formulations 23%, new manufacturing techniques 9%, grants 17% and other research 10%. Grants are made to institutes, hospitals and medical schools and include, for example, commitments such as the provision of $100,000 over three years to the Department of Medicine at the University of Melbourne for a First Assistant in clinical pharmacology. Another company is now

> contributing more than $70,000 annually to support the chair in clinical pharmacology and teaching in therapeutics in Australia". [APMA Fact Book, 1977:41].

It was suggested earlier that the DOP was part of a complex web of organisations. This social structure existed on a strongly contractual basis and to the extent that the DOP was extraneous from the point of view of the major exchange relations occurring in the local institution (and which were themselves subject to periodic review) it is being argued that the DOP was not as economically secure as the SSP. The relative complexity of the support structure of the DOP bears further investigation since it stands in considerable contrast to the SSP which, whilst being partially based on constructual bases (staffing and funding) was a fairly typical university based program. Not so the DOP.

There were four major organisations involved in the support of the Department of Clinical Pharmacology in which research on the DOP occurred: a large metropolitan Australian university, a multinational drug company, a large metropolitan Australian hospital, and a funding agency of the Australian government. The program members worked in one of the laboratories of the clinical pharmacology department of the university; the laboratory space and some of their equipment and services were provided by the hospital, and so physically speaking the laboratory was contained in a hospital rather than a university. In fact, although the head of the department and other senior scientists who worked in the two clinical pharmacology laboratories in the hospital, but who were employed by the university, had offices on campus, the clinical pharmacology department was defined by all concerned as existing in the hospital. The program was specifically funded by the Australian government, through the NHMRC, and by a multinational drug company. Part of this funding was for the provision of salaries: of the three research scientists and two research assistants involved, the program head was employed by the university, one senior research scientist was employed by the NHMRC, and the remaining research scientist and two research assistants were salaried by the drug company. The complexity of the program's support structure is related to the nature of medical training in Australia. Universities with medical schools are institutionally associated with "teaching hospitals", such that the university provides a large part of the theoretical section of a medical degree and the teaching hospitals provide a practical context for this theory, particularly towards the end of a medical degree. Thus, the clinical pharmacology department referred to in this thesis is a department in the medical school of a large university, but as mentioned, the research arm of the department is located in the hospital that performs as the major teaching hospital for the university. Nonetheless, given the traditional association between hospitals and university medical schools, the presence, in this case, of a university department in this particular hospital is not considered altogether unusual either by the hospital or the university.

The relations of exchange which provide the basis for the DOP were strongly professional in nature, and demonstrably affected the kinds of research conducted in the Department. A more detailed analysis of the social factors contributing to the establishment and evolution of the research goals of the DOP follows in a later section, but at this stage it is possible to make some general remarks about the nature of these exchange relations and the extent to which the medical and scientific goals and values which partially constitute the DOP were mediated by the various goals and values of the supporting organisations. This discussion of exchange relations will highlight a number of points of comparison with the SSP.

(i) Department of Clinical Pharmacology - Hospital: The hospital received technical services and advice from the department in exchange for the economic and social support it provided.[5] This meant that particular instruments, techniques and interests had to be cultivated by the department. Thus, the hospital provided a practically oriented medical context for an early interest in Parkinson's Disease (which was significant in the establishment of the DOP), and provided a continuing context for the constant appraisal of the activities of the clinical pharmacology department as being medically useful to the hospital. Although I cannot say to what extent the hospital provided specific mediating influences on the entire range of the Department's scientific and medical research, it is interesting to note that the DOP did not exhibit the effects of local institutional mediation in the way that other more practically oriented research programs in the department did. For example, the research that was concerned with drug testing in the department was oriented by the experimental population used: because the hospital provided the patients and overall patterns of drug therapy used on the patients the mediating effects of the local organisations on the scientific and medical goals of research related to the evaluation of drug therapy were much more apparent.

The effects of the local institutional mediation on the DOP were not the same however, because in the midst of an intensely practically and medically oriented environment the program was treated by all concerned as being predominantly "pure research": that is, the research was not expected to be practically relevant in the short term. This was despite the long term medical goals of the program. In other words, even if the hospital did mediate scientific and medical research in the department (by promoting particular intellectual interests, and by creating a generally medically oriented environment in and around the laboratory), the effects can only be readily discerned in the long term orientations of the program. The DOP was conducted virtually as a theoretical foil to the practically oriented work which occurred in the rest of the clinical pharmacology department. Thus, in the physical arrangement of the laboratory there was a clear separation between the "pure" end and an "applied" end - the DOP being worked out exclusively in the "pure" end of the laboratory. It was apparent that the program functioned for the program leader as a port in a storm of practical concerns.

In terms of professional orientation it appeared that a scientific professional orientational reference group was much more influential than a medical professional orientational reference group (this is borne out in the relative orientation of the components of the theoretical landscape and constellation of goals listed in Section 8.6).

The definition of basic scientific research was clearly separated by all group members from non-practice oriented medical research and therapy. This basic orientation contrasts quite markedly with the more highly practice oriented SSP.

(ii) Department of Clinical Pharmacology - Multinational Drug Company: The multinational drug company received the benefit of some drug testing services and a ready professional outlet for its products, but it mostly received the long term benefits of drug oriented research in exchange for the economic support it provided the department. As we have discussed, the drug industry invests in research on a large scale: the company concerned here has been estimated to spend in the order of $200 million annually on research, and although much of this research is in the nature of relatively long term testing of developed products and marginal modifications of existing products, there is still a large component of pure research performed by both company researchers and contract researchers. The DOP was conducted partially on the basis of a large recurrent grant by the company. The contract negotiated was for an initial period of ten years. Nonetheless, as mentioned earlier, the drug company did not apparently exert much pressure about the relevance of the Department's research to the company's interests. The main pressures exerted were that the research performed be of good quality and general pharmaceutical relevance. Towards this end there was an annual inspection of the laboratory by the drug company's international research director - in 1976 a seminar and exhibition day was arranged, and judging from the efforts to demonstrate the psychotic nature of octopamine crazed rats, and the other exhibitions of a more technical nature (including a molluscan "open heart" display), it seemed clear that the program leader, at least, was somewhat concerned that the research the company supported should be of interest to the company. The company's Australian employed researchers also followed the department's activities, but whether this was normal professional interest or company "snooping" is not clear.

Apart from the exhibition day described above there was little indication of a strongly institutionalised context of legitimation. Clearly, the basic research conducted by the members of the DOP required some legitimation (the complex funding arrangements indicate this) but insofar as the program was not strongly practice oriented the need to legitimate the program socially to non-scientists and sceptical peers was far less than in the case of the SSP, and as a consequence, criteria of evaluation external to those provided within the professional orientational reference group of science were not appealed to. Consequently, an institutionalised belief system comparable to the Solar Energy Belief System did not emerge and furthermore, as shall be demonstrated in the next section, was not available to the scientists.

(iii) Department of Clinical Pharmacology - University: The university received teaching and professional scientific and medical researcher training services in exchange for the economic and social support it afforded the department, and in many respects the clinical pharmacology department functioned like any department in a geographically dispersed campus. The biggest difference between this department and most other university departments in general was the institutional complexity of the clinical pharmacology department. There was a sense in which the department was the somewhat ad hoc creation of one man with the partial support of a drug company, hospital and university - the present support arrangements were clearly the result of compromises necessitated by the unwillingness of the university to give wholehearted support to a not yet proven discipline. In other words, the general immaturity, or low level of cognitive and social institutionalisation of the discipline of clinical pharmacology has both given rise to, and was reflected in the institutionalisation of the department.

The major mediating influence that the university had on the development of research generally was that it provided most of the effective research staff in the department (but not in the DOP) and consequently university interests were well represented, particularly through the obvious power of "hire and fire" and also through its effect as a major socialising agent for all of the student labour force. This mediating influence was however, diffused by the general complexity of the institutionalisation of the program (and this is taken to be an indicator of a low level of institutionalisation). This is illustrated by the employment arrangements in the department. In mid-1976, of the nineteen teaching, research and support staff members of the department, two were employed by the university as teaching staff, one was employed by the university as a part-time tutor, six were full time students, four were employed by the hospital, four were salaried from the drug company's funds, and the remainder were employed from other public and private research funds. That is, the university was the major source of staff in the department, but unlike other university departments, there were other major suppliers. The DOP had a slightly different mix. Of the six members, including one research assistant who was on extended leave, four were on drug company wages, one was on a university salary, and one was funded by contract with the NHMRC.

This difference in staff balance on the DOP did not mean however, that the drug company had any more influence over research. The overarching feeling about the funding of the program was one of impermanence: once the program money was used up, provision for further employment for all but the leader of the program was not secure. As a consequence there was no sense of sectarian loyalty to drug company, hospital, or even university (as a broad concept). The main source of solidarity was a general satisfaction with the department as a pleasant place to work in. This solidarity was very much a product of the somewhat charismatic personality of the department leader.[6]

Thus, in comparison with the SSP, the struggle for the survival of the DOP was a more directly economic struggle. The double bind situation that the solar energy researchers experienced and resolved as part of the general context of research was far more distant in the case of the DOP. Although the program members were conducting research partially under the influence of medical structures of relevance much less concern was expressed about the social relevance of the research being performed. The research was primarily defined by the researchers as *basic* research - this could be construed as "strategic" basic research by virtue of the program's medically oriented high level goals (such as "to develop a drug for the control of schizophrenia") but the prospect of social application of the research findings was a very distant possibility for all of the members of the program for the funders of the research, and presumably also for the broader scientific community of fellow researchers who were following the work in the DOP. That is, there was no significant conflict about the definition of the research: the DOP was basic research and by and large was considered legitimate as such.

7.4 Schizophrenia research in the context of legitimation

Summary: The context of legitimation of the DOP was less strongly institutionalised than that of the SSP. This was indicated by the absence of a coherent set of beliefs about the validity of the type of research being conducted on the DOP that compares with the Solar Energy Belief System. Researchers were able to resort to some beliefs about the validity of their research but these beliefs tended to be more scientifically than socially oriented (H4). For example, the one belief that was consistently referred to was a belief in the ultimately biochemical basis of "schizophrenia". In this section this belief is demonstrated to be only one of a range of ill-defined beliefs about schizophrenia that are entertained by researchers across a number of disciplines. This general lack of a consistent and coherent definition of schizophrenia indicates a generally low level of cognitive institutionalisation of schizophrenia research, and further supports

the contention that the context of legitimation of the DOP was not highly institutionalised.

7.4-1 Introduction

One of the higher level goals of the DOP was the development of a drug for the control of schizophrenia [G4 in Table 7.5-1]. As we will see, this goal has such broad scientific, medical and social implications that it provides a *clear* illustration of the way that cognitive and social aspects of the goals of research may not be easily separable, particularly when no strong division between a context of research and legitimation has been institutionalised (such as was the case with the SSP). Although this goal has been significant to research on the DOP its effect has been as a long term orientational factor rather than being immediately effective on particular research tasks. Thus, the goal was conceded to be effective by all the scientists on the program but it was not offered by them as the major reason for their research. Rather, the goal was considered to be important (by the program leader in particular) as an overall long term legitimation of the research, both to the general public and to the funders of research (particularly to the supporting drug company).

Research on the DOP was clearly based on a variety of beliefs and values related to the role of clinical pharmacology, and biochemically related research, but these beliefs were in no way as clearly articulated as the solar energy belief system discussed in the last chapter. This situation partially derives from the fact that basic research was considered (by researchers, funders and peers) legitimate as such and not in particular need of justification in terms of its immediate social relevance. This established legitimation appeared to offset the fact that the dopamine/octopamine theory of schizophrenia, together with the general emphasis that was placed on the role of dopamine and octopamine in brain function was a somewhat marginal theory amongst neurophysiologists. [7] But then again the established nexus of the discipline of clinical pharmacology with the pharmaceutical industry appeared to provide sufficient legitimation generally such that highly institutionalised belief systems were not necessary.

One consequence of this relative absence of a clearly institutionalised set of beliefs (which in the case of the SSP partially defined a context of legitimation) was the way that metaphysical beliefs tended to be incorporated into research in a fairly non-reflexive fashion. Of particular relevance to the DOP was the belief in a biochemical basis for all mental "illness". This was only one of many beliefs that were incorporated in the research - for example, molecular and biological reductionism, and a medical model of health, [8] but these latter beliefs are hardly unique to the DOP, since they tend to form part of a scientistic metaphysical basis that is shared by most practising natural scientists. [9] Insofar as there was a system of beliefs that particularly characterises the DOP (apart from the theoretical beliefs which partially constituted the context of research) the belief in the ultimately biochemical basis of schizophrenia was fundamental. This belief was taken for granted by all the researchers in the DOP and also in most of the relevant biochemical, medical and pharmacological literature examined in the course of the field work. Thus, all the researchers interviewed resorted to this belief when I questioned the validity of their research with respect to dealing with schizophrenia. That is, if the researchers were placed in a situation where they were effectively asked to legitimate their research they were able to resort to some beliefs about the validity of their research, but these beliefs tended to be more scientifically than socially oriented. For example, the one belief that was consistently referred to was a belief in the ultimately biochemical basis of schizophrenia. This belief expressed, in the final analysis, more of a commitment to molecular reductionism than a coherent analysis of schizophrenia - none of the researchers were in fact able to coherently define the nature of schizophrenia. The reasons for that failure will become abundantly clear as we go on to discuss the generally low level of cognitive institutionalisation of research about "schizophrenia".

In the remainder of this section I will show, through a brief historical sketch, that a belief in the biochemical basis of schizophrenia is only one of a number of different possible theories about the nature of schizophrenia. On that basis we will see that research on schizophrenia is not highly institutionalised. One consequence of this state of affairs was that the absence of seriously contemplated alternatives led to the unquestioned incorporation of a physicalistic medical model into the assumptions which underpinned research in the DOP.

7.4-2 Beliefs about the nature of schizophrenia

There is considerable evidence to suggest that societies have always been concerned with mental "illness". However, the way in which this concern has been expressed - firstly in theories about the nature and causes of mental illness, and secondly in practical measures adopted to treat that problem, appear to vary across cultures and also over time in particular cultures. [10]

In the West, theory and practice have followed particular paths that have been largely the product of a natural science oriented, industrial, capitalist society. Given the enormous theoretical and practical successes of science and technology in Western civilisation, particularly since the industrial revolution, it is not surprising that mental illness should have become the subject of both organised scientific research and a huge capital intensive pharmaceutical industry.

Until very recently the only seriously regarded approach in scientific research involved a medical-biological model of mental illness which entailed a search mainly for *chemical* causes of mental illness. As early as 1892 Kraepelin had suggested that mental illness is associated with a phenomenon he termed auto-intoxication, that is, a mentally ill person suffers an error in metabolism such that he creates in his own body a chemical compound which produces hallucinations, or which gives the appearance of physical or mental illness. A basic assumption underlying this approach is that the difference between mental health and mental illness rests with physical malfunction of the brain. However, at about the same time it became apparent that some distinction ought to be drawn between mental illness and mental disease - disease being, medically speaking, "bodily" disease [Gould's Medical Dictionary]. As the quote below illustrates this traditional distinction between mind and body, or in this instance between mind and brain, was reflected in a subsequent division of labour.

> "With the rapid developments in syphilology, psychiatry, and psychoanalysis during the first two decades of this century, there occurred a division of spoils, as it were, among them: paresis was claimed by syphilology, psychosis by psychiatry, and neurosis by psychoanalysis. The result was two reciprocal series of differentiations: patients became separated into paretics, psychotics, and neurotics; doctors into syphilologists (and neurologists), psychiatrists, and psychoanalysts (and psychotherapists). Separating the patients was, and still is, called making 'a differential diagnosis'. Separating the physicians was, and is still, called 'specialising' in the diagnosis and treatment of one or another branch of medicine". [Szasz, 1976:3].

This distinction between psychosis, neurosis and paresis is still drawn in contemporary research, although the distinction between psychosis and neurosis has more recently become somewhat blurred by a widespread scientific and medical realisation that schizophrenia, the most commonly identified type of psychosis, is a heterogeneous complex of pathological conditions rather than a uniquely specifiable illness. This blurring is in fact one of the major arguments in support of the claim being made in this section that scientific research (in general) about schizophrenia is at a rather low level of cognitive institutionalisation: the main point is that there is no scientific or medical consensus even as to what phenomenon is being studied. Most standard psychiatric textbooks, for example, reflect this lack of coherence of definition. To take an example from a standard Australian psychiatry textbook: Glasner and Solomon [1974:169, 171] start out in their section on "the Schizophrenias", by noting that "The American Psychiatric Association defines schizophrenia as 'a group of disorders manifested by characteristic disturbances of thinking, mood, and behaviour'" and go on to say that "there is no definite agreement concerning the fundamental causes of schizophrenia, and theories are most plentiful where the available data are least precise". The point is made well in another American textbook, Weiner [1967], who gives four different, but not inconsistent, definitions:

(a) "Schizophrenia is a disease that reflects a material defect in an organ system, perhaps the brain".

(b) "Schizophrenia is a physical illness that predisposes its host to limitations in adaptation and thus impaires his capacity with stress".

(c) "Schizophrenia is a psychosomatic illness".

(d) "Schizophrenia is a form of maladaptation

> originally occasioned by, and appropriate to, the family environment and its interacting members".

Sedgewick [1975] has presented considerable evidence that testifies to a confusion or at least a lack of clarity, in the theory and practice of schizophrenia research. This evidence supports the claim that schizophrenia research across all disciplines involved is not highly cognitively institutionalised. His arguments can be briefly summarised as follows:

(i) The "diagnostic paradigm" for the treatment of schizophrenia is strained by the fact that either diagnostic habits have changed over the years, or the character of the illness has changed since classical times (that is, the Kraepelin era), or both.

(ii) National "clinical cultures" exist, despite the fact that research directed towards proving the existence of a universal pattern of schizophrenic symptoms has revealed the "not very surprising news that hallucinated, deluded, rather withdrawn people can be found in a wide range of countries".

(iii) The distinction between disorders of affect and cognition postulated in traditional theory appears to be blurred.

(iv) The attempt to describe schizophrenia as a spectrum of pathological symptoms has entailed "psychiatric imperialism" over an increasing range of deviant behaviour.

(v) There is an apparent "physical imperialism" in the standard approach which from a very wide range of cognitive disorders initially excludes those with a known organic antecedent and then invites us to consider the remainder as possessing some organic antecedent.

Corresponding with this lack of coherence of definition there are a variety of disciplines and specialties which deal with schizophrenia. At the discipline level the obvious competitors are psychiatry, psychology, biochemistry, medicine, clinical pharmacology, and following the work of Bateson, Laing and Cooper, one might also add sociology. [11] At the specialty level it is possible to distinguish biological psychiatry, neuropharmacology (or psychopharmacology), epidemiology, clinical medicine, and the sociology of the family [cf. Kendell, 1975]. In other words, schizophrenia research is institutionalised in a variety of ways cognitively and socially. There is however, a long tradition of research into something labelled schizophrenia and so it seems a reasonable conjecture that generally speaking the support of such research is probably more highly organised than the concepts involved in the research, or that the research is more highly socially institutionalised than cognitively institutionalised.

Within this apparent confusion there may however, be more order than is immediately obvious. Sedgewick [1975] has identified three "paradigms" [12] of research into schizophrenic psychoses: the older medical-biological paradigm founded by Kraepelin and Bleuler, a more recent, "radical", completely social paradigm founded by Laing and Cooper, and thirdly, the now current medical-social paradigm, which is in effect a compromise between the first two approaches. This latter approach analyses schizophrenia in terms of an interaction between hereditary and environmental causative factors, such that particular schizophrenic events are considered to be the outcome of both a physical predisposition and an environmental social stress.

A low level of cognitive institutionalisation of research into mental illness has not entailed totally fruitless research though, for there have been a number of advances in the chemical treatment of neurosis and psychosis since 1952 and the initial epoch making isolation of reserpine from the plant Snakeroot (rauwolfia), and also in the same year the discovery that the drug chlorpromazine functioned as a tranquilizer as well as an antihistamine [McClure, 1973]. Since then a number of widely used drugs have appeared, in particular, the tricyclic and oxidase antidepressents and a variety of chemically varied but functionally related neuroleptics (that is, antipsychotic agents). However, according to VanPraag and Korf [1975] biological and biochemical research into depressive syndromes has been more productive than research into psychoses, which has been hampered by the inability to find a consistent relationship between the chemical structure of neuroleptics and their clinical effects. Contemporary neuropharmacological research has therefore been more directed towards the biochemical action profile of drugs rather than their chemical structure; this has led to the kind of research that occurred on the dopamine/octopamine program, which we shall examine more closely in the next section. Broadly speaking, the focus of neuropharmacological schizophrenia research is on the various chemicals involved in neuro-transmission systems in the brain. Research to date has focussed on three different chemical transmitters: dopamine, noradrenaline and seratonin. [13] It has not yet been established just these chemicals relate to mental illness in humans, although there are a number of hypotheses involving individual transmitters and combinations of transmitters [cf. Cools, 1975]. The dopamine/octopamine program had the working hypothesis that an abnormal concentration of a metabolite of dopamine, octopamine, was directly responsible for psychotic behaviour. This was only a working hypothesis however, and work on the basic mechanisms of pre- and post-synoptic transmission and reception of dopamine and octopamine was regarded as a more fundamental goal of research, such that in the event that no connection between octopamine and schizophrenia were demonstrated the goal would still retain theoretical legitimacy and interest.

In summary, the overall level of cognitive institutionalisation of research into schizophrenia is low. It is at the theoretical level that this is particularly manifest, where there is a lack of even a consensus over the phenomenon being investigated. This lack of coherence is reflected in the wide diversity of the social institutionalisation of schizophrenia research. There is not however, sufficient evidence available to be able to say that it is this diversity of social institutionalisation that is chiefly responsible for the low level of cognitive institutionalisation. What one can safety say however, is that the present lack of cognitive coherence is certainly preserved by the variety of disciplinary approaches that deal with schizophrenia.

Given this state of affairs it is hardly surprising that the researchers in the DOP were not able to say much more about schizophrenia, the ultimate object of their research, than that it represented "psychotic" behaviours that were difficult to control but which had, in the final analysis, a biological basis. A margin for error in the identification of these psychotic behaviours was agreed to be present, but this was not of great concern to the researchers who were constrained to focus their attention on behavioural analogues in rats. Provided some agreement could be reached as to what "psychotic" behaviour in rats was research need not be hindered by problems in the diagnostic paradigm. Further, even if the research were shown to be quite irrelevant to schizophrenia, the research could never completely lose its value as basic research about brain chemistry. This is of course, the ultimate defense of all basic research.

This belief in biochemistry as the ultimate basis for understanding and treating schizophrenia is not the only metaphysical belief that could have been explored as partially constituting the context of legitimation of the DOP. As mentioned earlier, others included beliefs in the validity of molecular reductionism allied with a more embracing medical model of health. These beliefs occur at such a general level of most scientists' consciousnesses however, that they are more accurately considered as part of a general scientific world view rather than a particular context of legitimation - not that they failed to be specifically evoked in particular contexts of legitimation when the occasion justified more general speculation or defense. As an exploration of the context of legitimation of the DOP however, our discussion of the biochemical origins of schizophrenia is quite adequate to demonstrate the main contention about this context of legitimation - that is, the context of legitimation of the DOP was less strongly institutionalised than that of the SSP; the major indicator of this is that there was no system of beliefs about the validity of the type of research being conducted on the DOP that compares with the "solar energy belief system" discussed in the last chapter. The general beliefs about the value of the DOP research formed more of a general background for the context of research. Legitimation, when it occurred on the DOP appeared to be on a rather more ad hoc basis.

Finally, it should be mentioned that the general level of knowledge about schizophrenia as a social issue (in the "common stock of knowledge", to use Schutz's term) is probably less than that relating to solar energy as a social issue. It will be recalled that the most likely source of public knowledge about schizophrenia as a social issue would have been the anti-psychiatry debate. [14] This debate was not, in the first instance, as high profile as the general subject of alternative energy options (particularly in the light of the current war between Iraq and Iran) nor is it at all of general contemporary interest, having faded into the mists of the sixties where it is unlikely to be revived by the perception of a relevant global crisis. Although I have no relevant empirical evidence it seems worth speculating that the level of institutionalisation of the context of legitimation of any research program will be affected by the constitution of the common stock of knowledge and perhaps also by the constitution of other relevant specialised stocks of knowledge that occur outside the world of science. Thus, because the level of public awareness about solar energy is higher than that concerning schizophrenia, the reference group "general public" (or even "particular pressure group") will be effectively more coherently defined and relevant to solar energy researchers acting in the context of legitimation that it would be to neuropharmacologists acting in the context of legitimation. Clearly, the level of articulation and coherence of any scientist's beliefs will increase in the context of a hypothetical (or real) dialogue with a well informed critic or adversary.

7.5 Goals and theory in the context of research

Summary: Research in the DOP occurred in the context of a structured cognitive field which consisted of interpenetrating theoretical, subject concern and technical levels. Similar to the last case study, two structures have been identified on the basis of empirical data: a theoretical landscape and a constellation of goals. These structures have been stratified into a disciplinary, sub-disciplinary and program level of research (H6). Inspection of the constellation of goals reveals that the members of the DOP were directed towards a variety of goals which occurred at different levels of the cognitive field of the research program (H8).

The two structures provided structures of relevance for research (H7) and were oriented towards two different professional orientational reference groups, one scientific and one medical in type (H1) - it will be recalled, in comparison, that researchers on the SSP were also directed towards two professional orientational reference groups, one scientific and the other engineering. Research in the DOP was, in

contrast to the SSP, more highly directed towards a scientific professional orientational reference group, which provided the scientists with relatively non-social criteria for their basic research. Like the researchers in the SSP, the members of the DOP also tended to quite generally bracket social considerations about their research as "external" to the research process (H3).

Table 7.5-1, the theoretical landscape of the DOP is a structure of specialised knowledge that formed the major part of the theoretical background for the research of the scientists involved in the DOP and which, in conjunction with the structure of goals, (also described in this section), provided structures of relevance for research. Compared with the SSP there are a similar number of components in the landscape, but the orientation of these components is more highly scientific in comparison with the SSP [see Table 8.3-4b]. The number of sub-disciplinary (including program level) components in Table 7.5-1 is however only seven compared with ten in Table 6.7-1. This is a product of a smaller number of program members with a more narrowly focussed basic research interest. Once again, the second round synthesis of the structure was accepted by the program members as adequate and this apparently rapid rate of convergence is taken as an indicator of a relatively highly institutionalised cognitive structure at the program level. It will be argued in the next section however, that (partly on the basis of a higher incidence of serendipitous events) the level of cognitive institutionalisation of the DOP members is lower than that of the SSP members.

TABLE 7.5-1: The theoretical landscape of the DOP.

Level of theoretical landscape	Theoretical components and their professional orientation*			
		Scientific		Medical
Discipline	T1		Clinical pharmacology	
	T2	Biology		
	T3			Medicine
	T4	Biochemistry		
Sub-discipline	T5		Neuropharmacology	
	T6		Neurophysiology	
Program	T7	Neural transmission theory, particularly the role of catecholamines and phenolamines as neurotransmitters in the nervous systems of: (a) invertebrates (viz: molluscs) (b) vertebrates (viz: rats) (c) man		
	T8	Dopamine theory of schizophrenia		
	T9	Dopamine/octopamine theory of schizophrenia		
	T10	Noradrenalin theory of schizophrenia		
	T11	Serotonin theory of schizophrenia		

* The components have been listed under their most influential professions. Where the components are oriented to both the professions they have been listed in the centre of the Table.

The constellation of goals presented in Table 7.5-2 was also based on a consensus after two rounds of negotiation. These goals are, unlike the goals of the SSP, much more exclusively scientific in orientation. The medical interests of the DOP are at a fairly high level and as I have suggested earlier, are long term goals that are in fact quite distant from the context of research.

Like the SSP, the DOP was a clearly defined sub-universe of meaning, quite separate from the "paramount" reality of a "common sense", broadly consensual reality. This scientific sub-universe was also strongly a-social in its structures of relevance - thus once again, a list of social factors which was postulated as relevant in the formation of the goals was accepted by the researchers with the barest of comment [see Table 7.7-1]. Generally speaking, the scientists and technicians consulted were not particularly interested in a more social type of analysis as relevant to their research interests.

Like the two similar structures discussed in the last chapter, these two structures provided motivational, thematic and interpretational relevancy for the researchers in the DOP. As we discussed in that chapter the structures need to be considered as interlocking structures which existed in the context of a process of research. Relevancy derived from these structures which were furthermore, partially the evolving products of a process of research as well as "external" constraints on research. The relationship of these structures to the various themes of research in the DOP will be described diagramatically in the section which follows where the goals, which are projections

TABLE 7.5-2: A list of theoretical and technical goals that effected the direction of the DOP (up to October, 1976).

Level of theoretical and technical goals		Professional orientation*			Approximate date of emergence of the goal**
		Scientific		Medical	
A. Theoretical goals					
Discipline	G1		To investigate the physiological effects of a broad range of drugs on man and other animals		1974
Sub-discipline	G2	To develop general models relating to: 2.1 brain function 2.2 drug action in the brain			April, 1975
	G3	To understand the biochemical basis of psychiatric disease			July, 1975
Program	G4			To develop a drug for the control of schizophrenia/psychosis	July, 1975
	G5			To develop screening processes for anti-psychotic drugs	July, 1975

* The components have been listed under their most influential professions. Where the components are oriented to both the professions, they have been listed in the centre of the Table.

** That is, emergence of the goal as significant in accounting for the direction of research.

TABLE 7.5-2 (cont.)

<table>
<tr><th rowspan="2">Level of theoretical and technical goals</th><th colspan="2">Professional orientation</th><th rowspan="2">Approximate date of emergence of the goal</th></tr>
<tr><th>Scientific</th><th>Medical</th></tr>
<tr><td>A. Theoretical goals (cont.)</td><td></td><td></td><td></td></tr>
<tr><td>Program (cont.)</td><td>G6</td><td>To elucidate the role of dopamine and octopamine in the biochemical mechanisms associated with schizophrenia/psychosis</td><td>July, 1975</td></tr>
<tr><td></td><td colspan="2">G7 To investigate an interesting chemical (ℓ-dopa) and its metabolites with particular emphasis on the anti-Parkinson and psychomimetic properties of ℓ-dopa</td><td>January, 1975</td></tr>
<tr><td></td><td colspan="2">G8 To elucidate dopaminergic and octopaminergic mechanisms and their role in schizophrenia</td><td>October, 1975</td></tr>
<tr><td></td><td>G9 To elucidate the biochemistry of a series of dopamine related chemical compounds that are involved in chemical transmission systems in the rat brain (and ultimately in the human brain)</td><td></td><td>April, 1975</td></tr>
</table>

TABLE 7.5-2 (cont.)

Level of theoretical and technical goals	Professional orientation		Approximate date of emergence of the goal
	Scientific	Medical	
A. Theoretical goals (cont.)			
Program (cont.)	G10 To develop a model of the pre- and post-synaptic mechanisms of dopamine and octopamine related neural transmission systems in the brains of:		
	10.1 molluscs		April, 1975
	10.2 animals (viz: rats)		July, 1975
	10.3 man		July, 1975
	G11 Specific research tasks following on from G4 (these research tasks were formally expressed in a recent - April, 1976 - NHMRC research grant application. The application was originally written in May, 1975):		October, 1975
	11.1 Precisely to specify the structural requirements for agonist and antagonist activity at the discrete post-synaptic receptors for dopamine and octopamine		

TABLE 7.5-2 (cont.)

Level of theoretical and technical goals	Professional orientation		Approximate date of emergence of the goal
	Scientific	Medical	
A. Theoretical goals (cont.) Program (cont.)			
	11.2 To define the nature of pre-synaptic mechanisms for the uptake, storage and release of octopamine and to compare these mechanisms with those known to be involved with other neurotransmitters		
	11.3 To define the activity of hallucinogenic and anti-psychotic drugs at the specific pre- and post-synaptic mechanisms for dopamine and octopamine and to study the effects of other psychomimetic drugs on these mechanisms		
	11.4 To design, synthesise and test compounds which may function as specific octopamine receptor blocking agents		
	11.5 To study the activities of compounds affecting octopaminergic mechanisms on the behaviour of animals		

TABLE 7.5-2 (cont.)

Level of theoretical and technical goals	Professional orientation		Approximate date of emergence of the goal
	Scientific	Medical	
A. Theoretical goals (cont.)			
Program (cont.)	11.6 To study the effects of compounds known to affect octopaminergic mechanisms on the storage, distribution and metabolism of octopamine		
B. Technical goals			
Program	G12 To develop chemical assays for:		
	12.1 dopamine		January, 1975
	12.2 octopamine		April, 1975
	G13 To synthesise deuterated neurotransmitters and related metabolites		October, 1975

from within the theoretical landscape, will be related to a process of research.

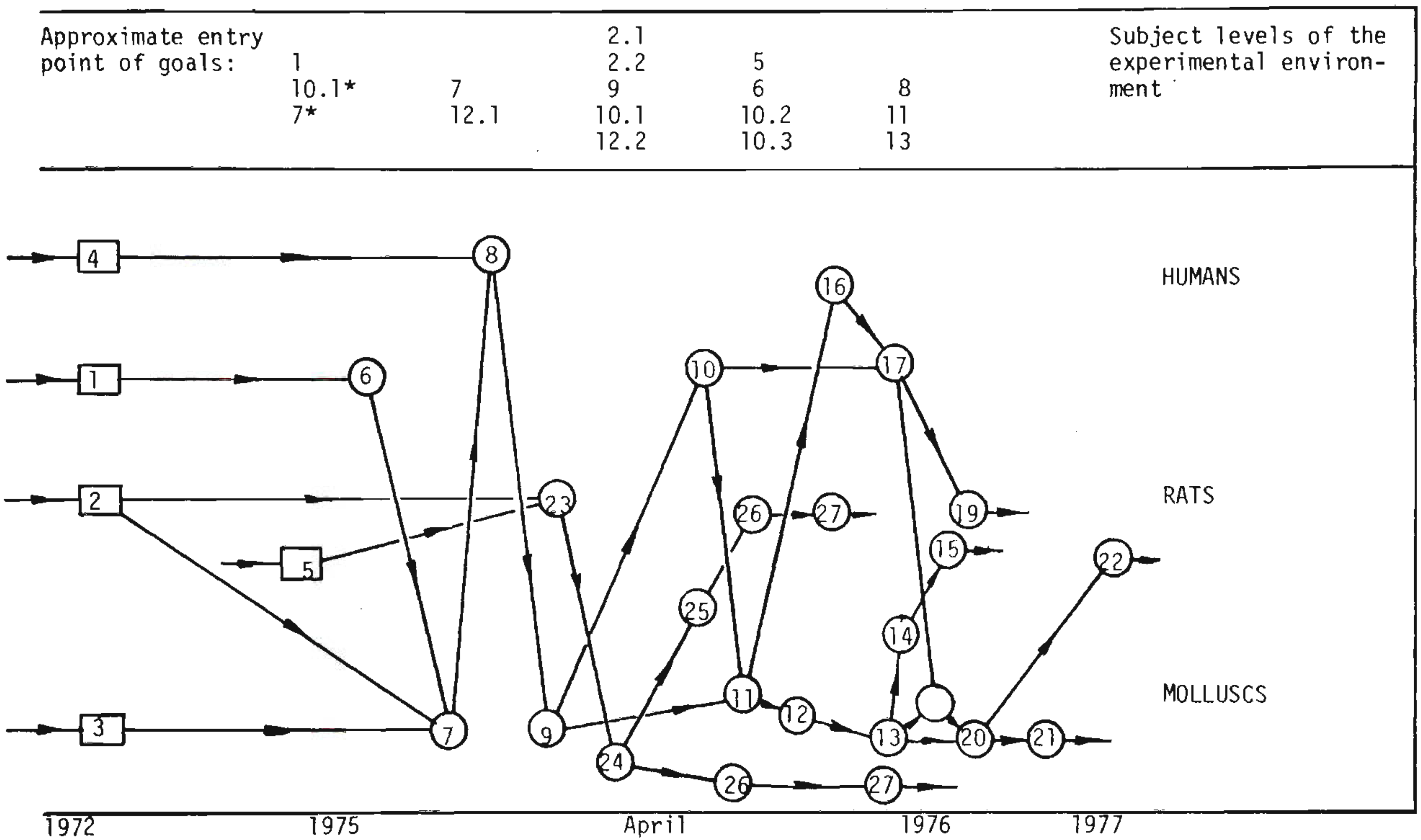
7.6 Goals and the evolution of research on the DOP

Summary: *The members of the DOP were constrained in their research by an evolving constellation of goals which provided a structure of relevancy (primarily in-order-to motivational relevance) for the scientists (H7). Compared with the SSP, research in the DOP was not highly institutionalised - the seminal theoretical hypotheses which linked dopamine and schizophrenia was said to have been serendipitous (like several other significant research events), and the establishment of the research program followed a path more of gradual evolution incorporating the resources immediately available than the simple adoption of pre-formed research strategies.*

Figure 7.6-1, a "flow diagram of significant research events in the evolution of the Dopamine/Octopamine Program" relates the goals listed in Table 7.5-1 to a process of research. The diagram pictures an evolving constellation of goals which provided a structure of relevance (primarily in-order-to motivational relevance) for the members of the DOP.

An interesting feature of the diagram is that the still, as yet, hypothetical link that was made between dopamine and schizophrenia. This link which followed the observation of psychosis-like side effects after the administration of ℓ-dopa to a patient with Parkinson's Disease (event 5) was defined by all concerned as serendipitous, that is, the

FIGURE 7.6-1: Flow diagram of significant research events in the evolution of the DOP.



* These goals had a very different context before the establishment of the dopamine/octopamine program.

KEY TO FIGURE 7.6-1: Flow diagram of significant research events in the evolution of the DOP.

Note: For greater ease of comprehension this list has been arranged to demonstrate something of a logical development of events. This reconstructed logic is sometimes at the expense of the chronological sequence of events.

PROGRAM MEMBERS* AND THEIR ESTABLISHED LINES OF RESEARCH:

1. Lance: Clinical studies of Parkinson's Disease.

2. Denise: Drug absorption and protein binding.

3. Oliver: Dopamine receptors in molluscan tissue.

4. Peter: Clinical Studies of Parkinson's Disease.

5. Angela: Uptake and release of amines in nervous tissue.

PARTICULAR EVENTS:

6. Adverse side effects (psychosis-like) were observed after the administration of ℓ-dopa to a patient with Parkinson's disease - it appeared as if the normal ℓ-dopa effect was being blocked somehow. The patient had also been given the gastric emptier metoclopramide.

7. Metoclopramide demonstrated to be a dopamine antagonist.

8. It was hypothesised that dopamine antagonists might be derived from dopamine itself and produce the "on-off" effect in Parkinson's disease.

9. Work on dopamine metabolites in molluscan and mammalian tissue.

10. The existing dopamine hypothesis of schizophrenia began to be questioned and reformed. It was hypothesised that schizophrenia might be due to a failure to produce a dopamine antagonist.

11. It was hypothesised that there are multiple sites for the reception of dopamine. Attempts were made to sub-divide

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* Pseudonyms have been used.

KEY TO FIGURE 7.6-1 (cont.)

dopamine receptors in the mollusc and to test for blocking by anti-psychotic drugs.

12. Multiple sites found for the reception of dopamine in molluscs.

13. The anti-psychotic drug Clozapine did not act as a dopamine blocker on one particular dopamine receptor site.

14. An investigation for multiple sites for dopamine reception in rat brain was desired.

15. Conceptual and technical difficulties (such as the lack of suitable dopamine agonists) halted this line of research.

16. It was hypothesised that there are multiple sites for the reception of dopamine.

17. The dopamine/octopamine hypothesis: it was hypothesised that anti-psychotic drugs might block octopamine or related phenolamine metabolites of dopamine, and that this blockade might be responsible for anti-psychotic effects. (This followed from the observation that anti-psychotic drugs didn't appear to block any specific dopamine receptor, that is to say, there didn't appear to be any one specific receptor associated with anti-psychotic effects).

18. Clozapine demonstrated to block octopamine in molluscs. (This work followed from the hypothesis that since Clozapine was a very weak dopamine antagonist, its anti-psychotic effects might be due to the blockade of another unknown neurotransmitter).

19. Rats adopted defensive posturing when injected with octopamine or octopamine precursors. The effects could be blocked by Clozapine. (This was taken as supporting evidence for the importance of octopamine in psychosis/schizophrenia).

20. Specific receptor sites for octopamine found in molluscan neural tissue.

21. Data collected to support an octopaminergic neuronal pathway in molluscs.

22. Indirect evidence of two specific dopamine sensitive receptor sites in rat tissue was received from overseas - "it was then that we realised we weren't alone".

23. Work on the uptake and release of dopamine from rat brain.

KEY TO FIGURE 7.6-1 (cont.)

24. Octopamine found in large amounts in the neural tissue of molluscs.

25. Major technical problems experienced with the working up of octopamine assays to detect small amounts of the amine.

26. Major review and systematisation of the present status of research undertaken.

27. Development of chemical assays based on mass spectrometric techniques. This entailed the synthesis of deuterated dopamine metabolites.

hypothesis was a "happy accident". And, of course, insofar as the link actually surprised the two people concerned one can hardly dispute their experience of the event as serendipitous.[15] This serendipitous hypothesis was in fact, the inspirational idea behind the eventual formalisation of the program around the dopamine/octopamine hypothesis (event 17). The event was, so far as the scientists on the program were concerned, unique in its inspirational boldness. This was not the only serendipitous event of major significance however; four other events were described by the scientist most closely involved with them as serendipitous - events 12, 13, 18 and 20. All of these events played an important role in the development of the program. This relatively high level of serendipity compared with the SSP is taken to be an indicator of a relatively lower level of cognitive institutionalisation. Thus, in the early stages of the program the degree of latitude available to a study of dopamine as an "interesting" chemical was enormous and consequently the connection between schizophrenia and dopamine, whilst on the one hand being just one of the possible permutations of parameters available from locally ongoing research, was still "surprising" in that there was obviously no clearly defined pre-existing structure of ideas or research activity immediately available to act as a guiding context. Nonetheless, it is quite clear that the immediate availability of particular personnel, instruments, techniques, etc., in the local environment of the program leader was also a major influence on the selection of the dopamine/octopamine transmission system as worthy of continued study. Furthermore, if the work on molluscan systems had not been available, together with the scientist responsible, it is unlikely that the program would have developed at all, given the wide range of theoretical possibilities available at the time. This wide range of possibilities was a consequence of the low level of social and cognitive institutionalisation of both the discipline of clinical pharmacology and the specialty of neuropharmacology. Subsequently, however, with the establishment of a variety of goals as directives, the securement of funding and the subsequent hiring of personnel, the procurement of equipment, and the gradual establishment of a program of research within cognitive and social structures with increasing levels of institutionalisation the range of cognitive *and* social choice available was decreased.

The original range of choice available in the possible direction of research was very much a product of the variety of the professional orientational reference groups that affected the research. This diversity, which contrasts in some ways with the SSP is best appreciated through an appreciation of the professional socialisation of the most influential scientists and the relationship of their research interests to the research program.

The professional training of the scientists on the dopamine/octopamine program was quite diverse, covering two major professions - medicine and science - and several disciplines: biology, biochemistry, chemical pharmacology, chemistry and medicine. This diversity in backgrounds is in contrast with the more unified physics background of researchers in the SSP and is significant in understanding the evolution of the research in the DOP.

The program leader of the DOP was both a Ph.D biochemist and a medical doctor, and whilst he conducted his research on the program as a scientist, much of his work in the laboratory was influenced by his medical training. [16] Thus, it was mainly through some jointly conducted medical research into the treatment of patient with Parkinson's Disease that an initial link between dopamine and mental illness was established as being worthy of further research. This medical research was centred around the use of ℓ-dopa as a drug, and occurred in the context of other research on biochemical aspects of dopamine. This research which concerned biochemical mechanisms of dopamine metabolism in molluscs was being conducted by the (later) senior research scientist and was an extension of his biochemical Ph.D research. This early research on dopamine was performed while the senior research scientist was employed by the Sydney laboratories of a drug company other than the one that eventually partially supported the dopamine/octopamine program. After a period of part-time employment in the clinical pharmacology department, the senior research scientist was employed by the NHMRC as a full time research officer on a program concerned with dopamine antagonism in Parkinsonism (this was the precursor program to the DOP). He had the role of an "ideas man", partially due to a wide ranging intellect, but also due to his employment as a full time senior scientist on the dopamine/octopamine program. The other research scientist employed on the program was a newly graduated Ph.D clinical pharmacologist.

Her Ph.D work was supervised by another scientist in the laboratory but she subsequently did some research with the dopamine/octopamine program leader on various aspects of drug absorption. Her work on the program flowed on from Ph.D work on the labelling of various sites of drug reception in body tissue but this earlier research was far less concerned with neuropharmacology than with general biochemistry.

In conclusion, it seems likely that the diversity of the interests and research backgrounds of the members of the DOP is a rough indicator of the relatively low level of cognitive institutionalisation of the research program. It is likely, furthermore, that this diversity was an advantage to the research insofar as diverse backgrounds were an encouragement to intellectual receptivity - receptivity being important, generally, where established guidelines are not abundant.

7.7 Some important characteristics of the process of formation and evolution of the research goals

Summary: *Similarly to the SSP, research on the DOP was found to be constrained by social, economic and political factors. This was demonstrated through an analysis of various social aspects of the process of formation and evolution of the research goals of the program. In contrast with the SSP however, the goals of researchers in the DOP did not appear subject to any significant changes in relevancy. Although the more general goals of the program were partially established in a context of legitimation they did not become as highly separated from the context of research as did the higher level goals of the SSP (H9). This is interpreted as indicating a low level of institutionalisation of the context of legitimation of the DOP.*

The constellation of goals which oriented researchers in the DOP was a stable, cumulative structure which evolved in conjunction with

the research by a process of sedimentation of new goals. The goals were not subject to sudden change as a consequence of the emergence of significant research events (H10). There were, however, indications that the level of cognitive institutionalisation in the DOP was lower than that in the SSP. These were a higher level of serendipity and a greater degree of movement between different subject levels of the experimental environment in the DOP.

The research of the members of the DOP became increasingly technical in orientation as the program evolved (H11).

Similar to the SSP the goals of research of the DOP evolved in the context of social, political and economic factors which were significant in influencing the context of individual goals and in providing a broad structure of motivational relevance for researchers in the DOP (the primary source for this Table is contained in Figure 4 in Appendix 12). Again it is stressed that all the socially oriented factors provided a general matrix of relevance for all of the goals - despite the fact that single goals have been singled out as uniquely important. Here again the main criterion is that of relative degree of effect rather than uniqueness of effect. This structure will be discussed further in the sub-sections which follow.

The social factors shown in Table 7.7-1 have been listed in three categories: general professional considerations, political/ economic strategies primarily at the school and departmental level of organisation within the university, and considerations relevant to career and personal advancement. The factors listed are quite similar in nature to those listed for the SSP. One major difference however, was the overt intrusion of ethical considerations in the testing of certain

TABLE 7.7-1: Summary of socially oriented factors which were particularly significant in the establishment and evolution of the goals of the SSP.

Significant socially oriented factors*		Goals (in approximate order of evolution)**																
		1	7	12.1	2.1	2.2	9	10.1	12.2	3	4	5	6	10.2	10.3	8	11	13
A. General professional considerations	1													x				
B. Political/economic strategies primarily at the school and departmental level of organisation within the university	2	x			x	x				x	x				x			
	3(i)	x				x				x	x	x						
	(ii)	x				x				x	x	x						
	(iii)	x				x				x	x	x			x			
	4(i)		x	x	x	x				x	x	x		x		x	x	
	(ii)		x										x				x	
	5										x							
C. Considerations relevant to career advancement/personal advancement	6(i)							x					x			x		
	(ii)			x							x	x						
	7(i)		x															
	(ii)		x															
	8							x										

* See Table (cont.) for a list of these numbered factors.
** See Table 7.5-2 for details of the goals.

TABLE 7.7-1 (cont.)

A. General professional considerations:

1. Ethical considerations require this goal.

B. Political/economic strategies, primarily at the school and departmental levels within the university:

2. Research that fulfils at some levels, both scientific and medical criteria of relevance provided general legitimation for the existence of the research program.

3. Research with medical/social/economic potential was considered important for:

   i. The further legitimation of the discipline of Clinical Pharmacology.

   ii. The further legitimation of the Department of Clinical Pharmacology.

   iii. The raising of funds for further research.

4(i) A gap in the existing state of knowledge was perceived. This gave the research program strategic value in the solutions to recognised scientific and/or medical problems.

(ii) Research with scientific potential was considered important for the raising of funds for further research.

5. Success with this goal might provide economic returns through royalties for the university and marketable product for the drug company.

C. Considerations relevant to career advancement/personal advancement:

6. This goal was seen to be potentially fruitful from the point of view of production of:

   i. publishable results;
   ii. a novel product/process worthy of being developed for more general use.

7(i) An established line of fruitful research might be profitably expanded.

TABLE 7.7-1 (cont.)

(ii) This research was a major reason for employing particular personnel.

8. A potentially fruitful Ph.D project might be profitably expanded.

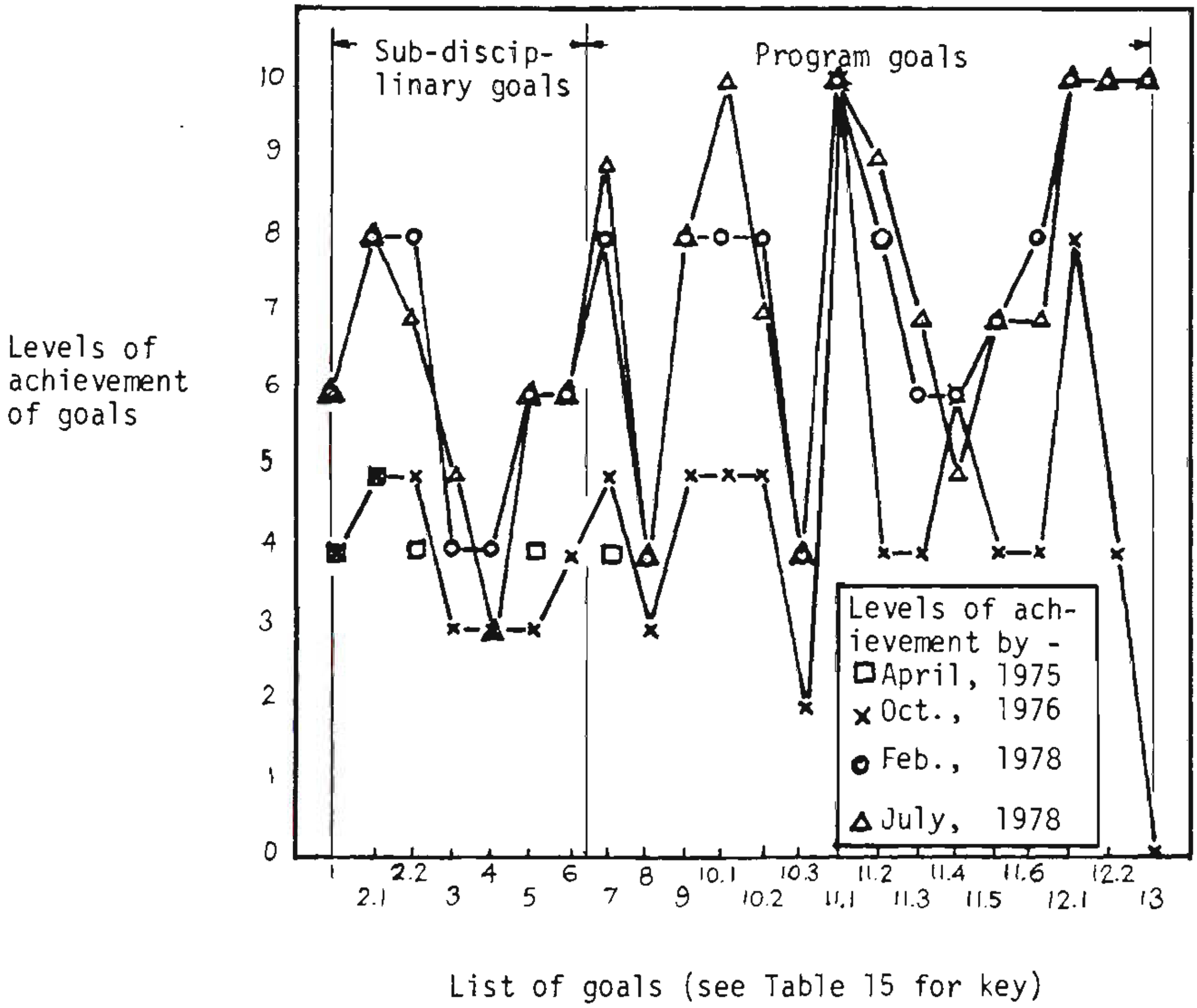
hypotheses and results - the data obtained from molluscan systems was tested on rats rather than human beings (which were the "ultimate" objects of the molluscan experiments). The biological systems of laboratory animals are of course, never fully identical to those of humans, but in general animals tend to be preferred to human subjects for reasons of cost and the relative sanctity of human health and freedom.

Within this context the goals that were listed in Table 7.5-2 formed a very stable structure. As with the goals of the SSP, none of the goals were said to have changed over the period, and the priorities of the researchers remained very stable over the period investigated. This stability was associated with a number of features:

(i) The more general goals were partially established in a context of legitimation but at the same time remained part of the context of research:

The more general goals of the program (goals 1-6) were all established within the first year [see Figure 7.6-1], but in contrast with the more technical goals which were established later the rates of progress towards these goals was generally lower [see Appendix 15]. The levels of achievement of the more general goals were not markedly different however [see Figure 7.7-1]. This is in contrast to the situation on the SSP where the more general goals tended to become fully achieved in the minds of the researchers and displaced to a context of legitimation. This is taken as an indication of the way that a context of legitimation was not as highly institutionalised in the DOP as it was in the SSP. Nonetheless, the more general goals were certainly *partially* established in a context of legitimation. Thus, because the distinction between research and legitimation

FIGURE 7.7-1: Levels of achievement of the goals of the DOP (group averages)*



* Note: These graphs have been consensually accepted as accurate representations of the levels of achievement of the goals. The mean deviation of the first round responses do however, give some indication of the extent to which individual perceptions did vary initially. These deviations vary slightly from curve to curve but are in general of the order 2.0 units [see Appendix 15 for details].

was not clearly defined (due to less pressing legitimation needs) it is not possible to identify a clearly defined context of legitimation with uniquely specified goals that are clearly separated from the process of research.

As was the case in the SSP, the legitimation process in the DOP had two important aspects:

Firstly, legitimation to "significant others" who might be needed to provide economic, political and scientific support, and secondly, "internal" legitimation involved in the process of the formation of a research group that felt itself to have an identity (that is, intellectual and social coherence - not that these two aspects are fully separable).

Similarly again, these first six goals were agreed by the researchers to be less concerned with matters that were immediately relevant to career advancement and personal advancement than were some of the other goals. That is, although this latter category is certainly not separable from political or economic considerations, goals 1-6 were felt to be, in comparison with other goals, more generally associated with issues of long term strategy. These issues which are listed below, were then, not necessarily based in the immediate interests of the members of the DOP:

1. Research that fulfilled at some levels, both scientific and medical criteria of relevance provided general legitimation for the existence of the research program.

2. Research with medical/social/economic potential was considered

important for:

(i) The further legitimation of the discipline of Clinical Pharmacology;

(ii) The further legitimation of the Department of Clinical Pharmacology;

(iii) The raising of funds for further research.

3. (i) A gap in the existing state of knowledge was perceived. This gave the research program strategic value in the pursuit of solutions to recognised scientific and/or medical problems.

It will be recalled that the more general goals of the SSP tended to be imposed on program members by authoritative outsiders. In addition, these imposed goals were associated with a lower "autonomy index" than the more technical goals. The more general goals of the DOP were not imposed by outsiders, but as Figure 7.7-2 shows, they were nonetheless, imposed "from above" by the program leader to a greater extent than was the case for the other goals. The autonomy index for these goals of the DOP is, however, closer to a balanced state of a relative individual autonomy (that is, 0 on the index scale - see Appendix 1 for further details) when compared with the other goals [see Figure 7.7-4 and Appendix 20]. This seems somewhat anomalous with the information contained in Figure 7.7-2 which might suggest that the greater degree of shared influence over the formation of the more technical goals would entail a greater level of autonomy (on average). The higher priorities and levels of involvement with some the more general goals do however tend

FIGURE 7.7-2: Maximum influence of individuals on the goals of the DOP

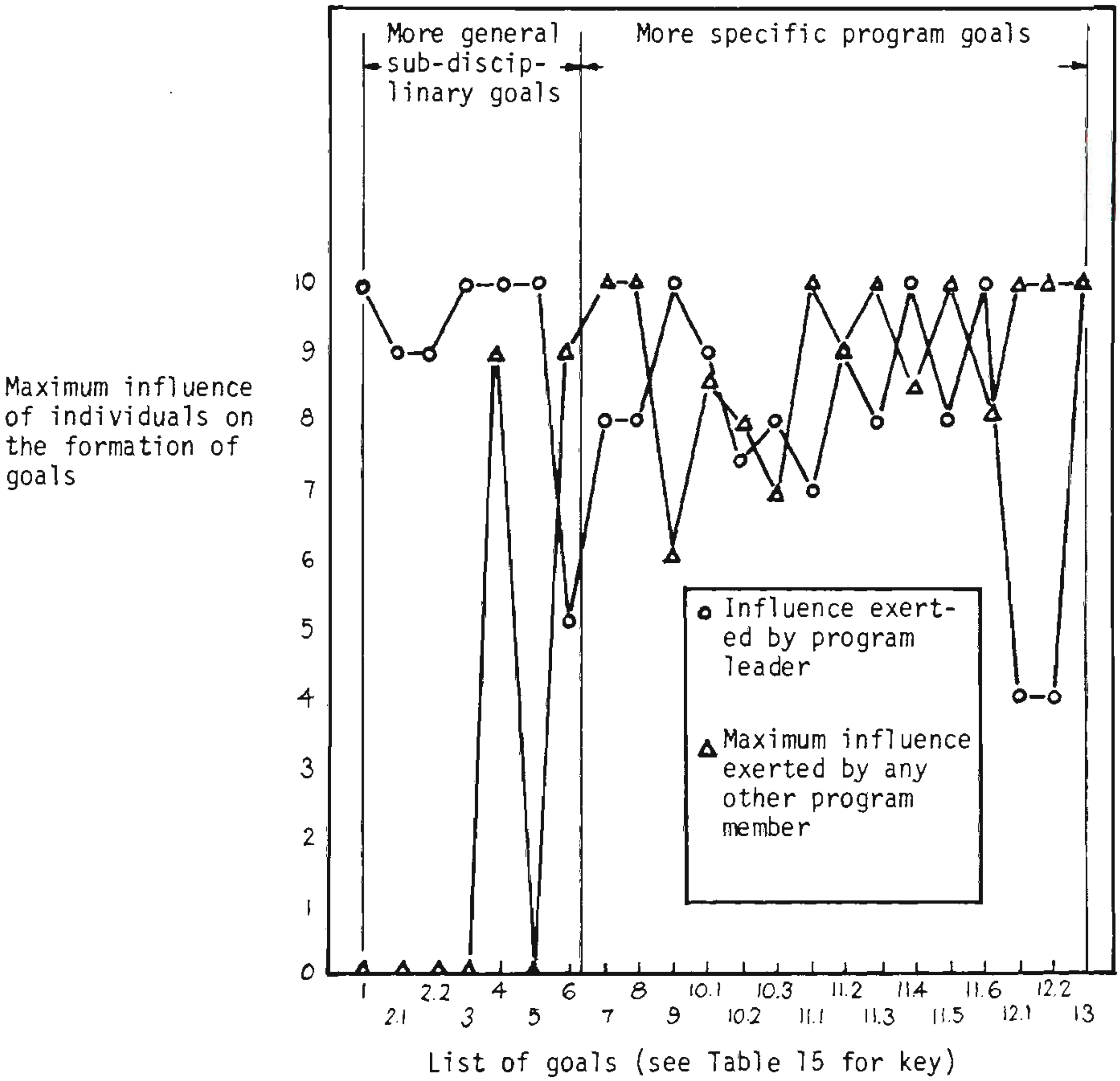


FIGURE 7.7-3: Average priorities of the core groups for particular goals of the DOP.

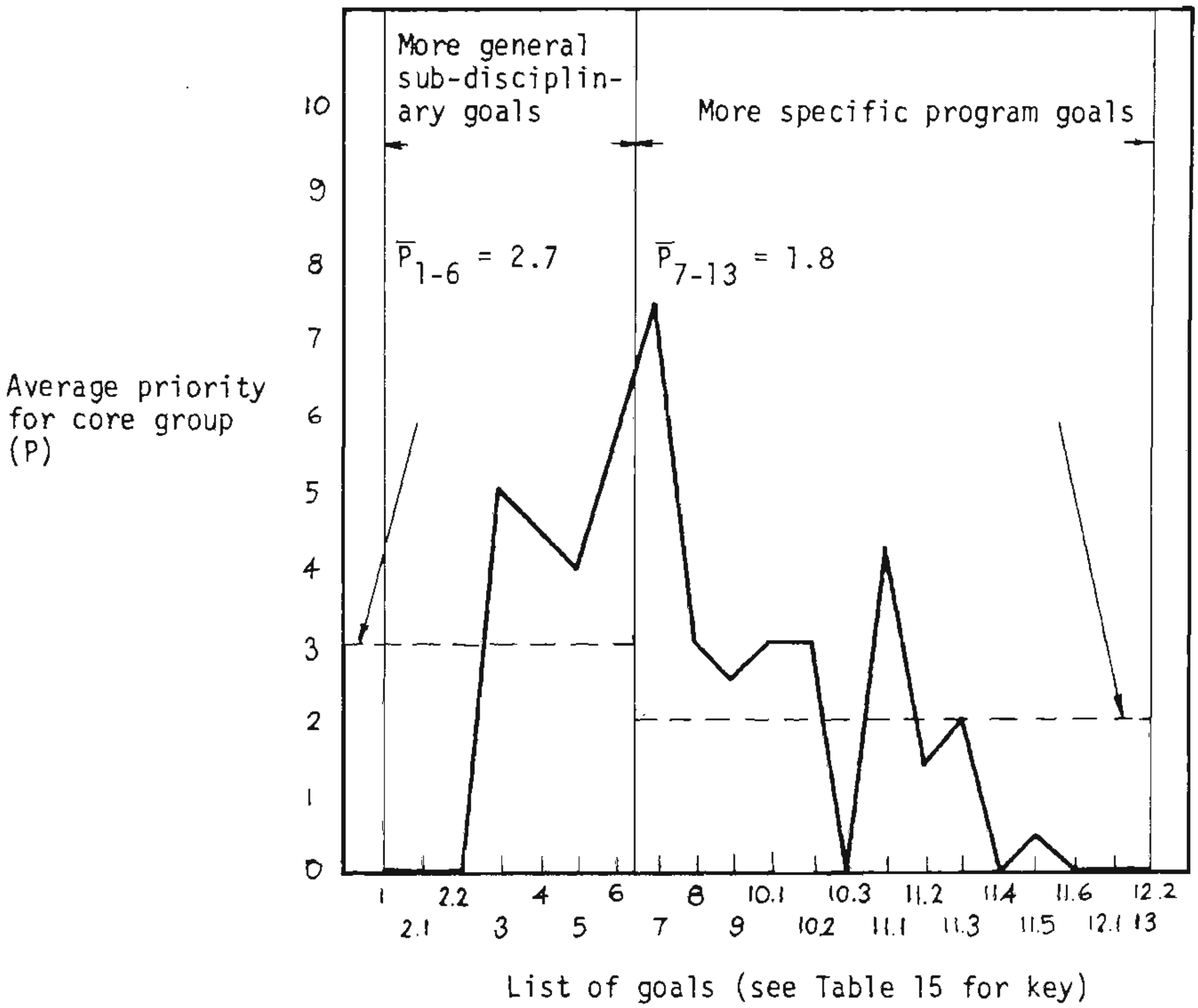
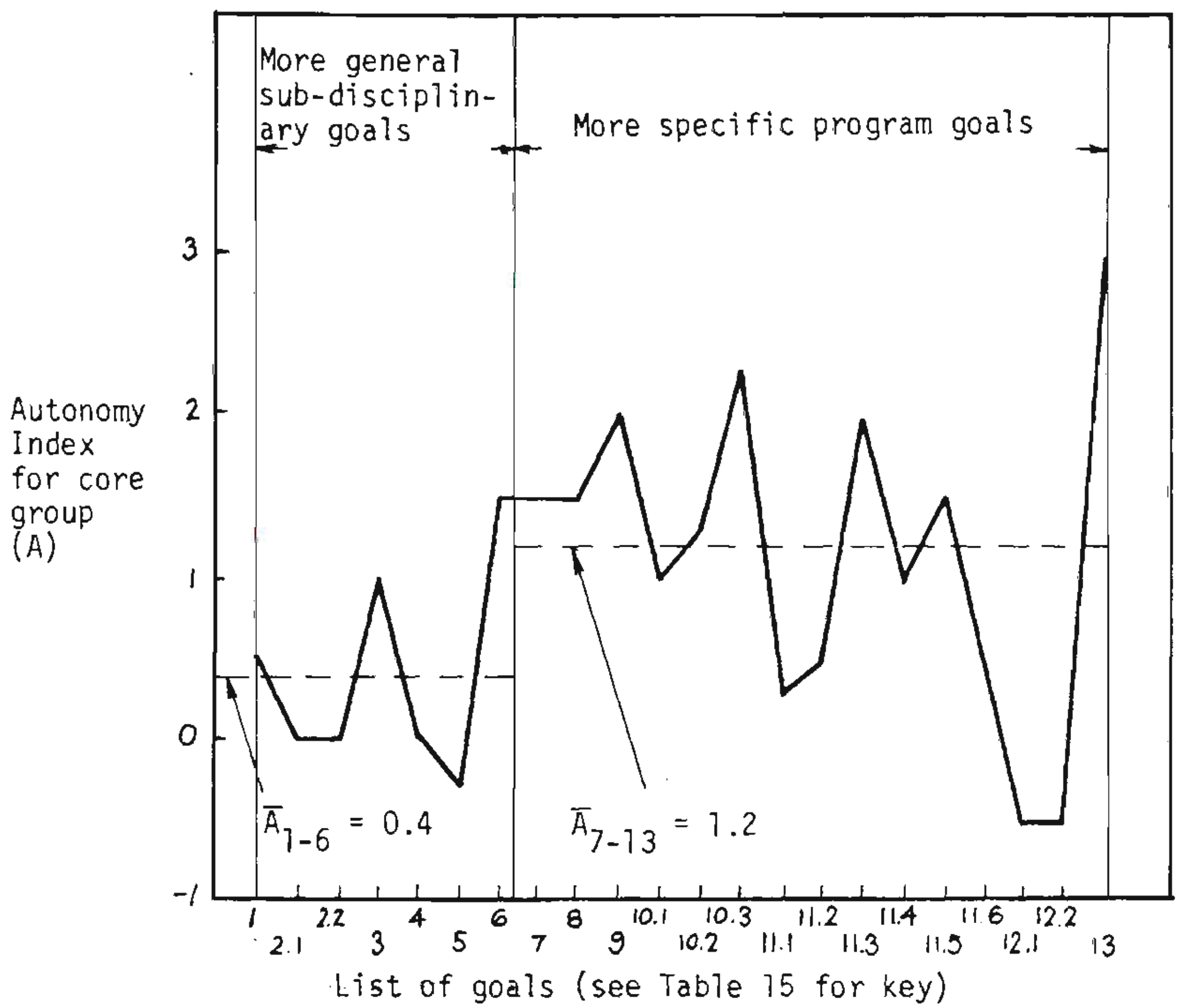


FIGURE 7.7-4: Autonomy indices for core group of the DOP



to offset this apparent loss of autonomy. All of this evidence supports the original contention that the more general goals of the DOP tended to remain in the context of research along with the more technical goals, even if they were somewhat distanced from the reality of day-to-day research. This is in marked comparison with the situation on the SSP.

(ii) The constellation of goals was a stable, cumulative structure which evolved in conjunction with the research in a process of sedimentation of new goals:

There were no dramatic changes in the nature of the goals as a product of the research. Rather, a gradual evolutionary process occurred with most of the goals being established before the crystallisation of the major research hypothesis. That is, 18 of the 21 goals that were identified were established before the formalisation of the dopamine/octopamine hypothesis (event 17). This is similar to the process of evolution of research on the SSP in that 17 of the 24 goals were established there before the occurrence of two critical research events.

On this basis it appears that the constellation of goals shared by researchers was in both programs a stable structure which was highly effective in providing direction for research. The evidence suggests further, that in both cases the research was highly pre-planned, although as discussed in the last section, and as will be discussed in more detail in the next sub-section, there are some indications that the level of cognitive institutionalisation in the DOP was somewhat lower than that present in the SSP.

(iii) The pattern of research events in the DOP was however, more "open" than events in the SSP. There are two main indicators of this "openness":

(a) The number of serendipitous events which occurred in the DOP was greater than for the SSP (five as opposed to one, events 6, 12, 13, 18 and 20 in the DOP and event 28 in the SSP).[17]

(b) There was a greater degree of movement between different subject levels of the overall experimental environment. That is, on the flow diagram of the DOP there is a considerable amount of "tacking" from one level of "biological system focus" to another. Thus, any research event tended to have fairly immediate consequences at other levels of experimentation. This is in contrast to the evolution of events on the SSP which tended to be more linear and restricted to the level of origin of the preceding event. These differences in the extent of "cross fertilization" between experimental levels can be partially explained in terms of the amount of "interpenetration" of different program member's lines of research. If the two flow diagrams are compared, it can be observed that in the DOP the research of all of the program members co-ordinates in event 8 in Figure 7.6-1 some three months after the beginning of the program. In comparison it took three years and three months before all the members of the SSP co-ordinated in event 25 in

Figure 6.8-1. This result would seem to indicate a much greater level of "individualism" in the research on the SSP. A possible reason for the lower level of individualism in the DOP lies with the relatively higher degree of direction exercised by the more charismatic leader of the DOP, but the data is somewhat inconclusive - for example, the autonomy index figures for the more technical goals indicate the opposite result, that is, that the members of the SSP felt less personal control over their research than the members of the DOP.

A brief comparative analysis of the numbers of inputs to, and outputs from, the research events in both programs has been attempted in the next section [see Table 8.3-3(g)] but the results there are also inconclusive. Nonetheless, the two observations about the relative occurrence of serendipity and the amount of movement between system levels are strong evidence in support of the postulated lower level of cognitive institutionalisation of the DOP compared with the SSP. A summary of the various indicators that have been used to compare these levels of institutionalisation is presented in the chapter which follows.

(iv) As the DOP program evolved the research goals became increasingly technical in orientation, this being a natural consequence of the increasing crystallisation of hypotheses and research techniques:

This increasing technical orientation over time is a similar tendency to that observed in the SSP. If the orientation of the group's publications is used as an indicator of this tendency, it is apparent that the tendency was much more marked on the SSP - something to be expected in the light of the greater practice orientation of the SSP. A considerably larger proportion of the publications of the DOP group were oriented towards the more general goals - 25% of the group's 20 publications over the period 1975-77 were directed towards goals 2.2, 4, 5 and 6 [cf. 11% for the SSP]. That is, the publications of the DOP group were not as restricted to a technical level. Some publications were only directed towards more general goals (unlike the publications of the SSP group). The most popular goals were still technical, however - 55% of the DOP group's publications were focussed on two goals (goals 8 and 10.1). In comparison all of the technical SSP group's publications were directed towards one or other of 5 technical goals. Further details of the group's publications record are detailed in Appendices 16 and 17.

(iv) The publication record of the DOP group reflects the low level of cognitive and social institutionalisation of clinical pharmacology and the DOP:

From the group's publication record it is clear that the professional audience for the results of clinical pharmacological research in general is by no means a uniquely defined group and consequently there are a large number of journals with divergent interests which are potentially available as publication outlets. The publication record of the group members taken as a whole is interesting in this respect. Of the overall total of sixty-one publications (including articles in books, full papers, reviews and abstracts) thirty-two different publication outlets were used. The thirty-one full papers that have been produced by the members in their professional lifetimes have been published in no less than eighteen different journals. (In comparison the SSP program members used 12 different journal outlets for their publications). The publications that can be classified as occurring in the phase of research that immediately preceded the dopamine/octopamine program demonstrate a little more selectivity however. Of the seventeen publications, all of which were joint publications, only six different outlets were used. This represents an increase in the frequency of publications in a particular place of approximately fifty percent. In itself, this brief analysis does not preclude the existence of a well defined audience of selectively reading professional specialists all dealing with similar problem areas to the dopamine/octopamine program members, but as reflected in the publication practices of the program members, it seems most likely that the relative novelty of the particular approach being used on the program, and the relative novelty of the major parent discipline of clinical pharmacology does preclude highly developed, coherent specialisation.

7.8 Conclusions

In this chapter, further support has been given for a number of the hypotheses that were first assessed in the last chapter.

The group of pharmacologists studied was shown to perform research as part of a research program which was constituted through the collective activities of a group of research workers who shared a commitment to particular research practices and techniques, who were directed in their research towards a shared set of goals, and who shared, to some extent, a common stock of knowledge (H12).

Researchers in the DOP were subject to the social and cognitive controls of professionalism which operated through the agency of the professional orientational reference groups of science and medicine (H1). These reference groups provided a basis for the scientists' distinctions between, and definitions of scientific and non-scientific activity. Research in the DOP was, in contrast to the SSP, more highly directed towards a scientific professional orientational reference group, which provided the scientists with relatively non-social criteria for their basic research (H2). Like the researchers in the SSP, the members of the DOP did, however, tend to quite generally bracket social considerations about their research as "external" to the research process (H3).

Research in the DOP occurred in the context of a structured cognitive field which consisted of interpenetrating theoretical, subject concern and technical levels. On the basis of empirical data, two structures were identified: a theoretical landscape and a constellation of goals. As in the last case study, these structures were stratified into a disciplinary, sub-disciplinary and program level of research (H6). These two structures provided structures of relevancy for research (H7).

Inspection of the constellation of goals revealed that the members of the DOP were directed towards a variety of goals which occurred at different levels of the cognitive field of the research program (H8).

This constellation of goals was, like the constellation of goals in the SSP, a stable, cumulative structure which evolved in conjunction with the research by a process of sedimentation of new goals (H10).

As with the SSP, research in the DOP became increasingly technical in orientation as the program evolved (H11).

On the basis outlined above the DOP was quite similar to the SSP - indeed it would be surprising if any research program did not largely share the characteristics outlined above. There were however, significant differences between the two programs:

The DOP was by virtue of its very basic orientation not nearly as oriented towards social application as the SSP (H13). Research in the DOP was as a consequence, far less constrained by considerations of the economic feasibility of research products, although researchers were nonetheless constrained by social factors as analysis of aspects of the process of formation and evolution of the research goals revealed (H14).

The context of legitimation of the DOP was less highly institutionalised than that of the SSP. There was no clearly articulated belief system relevant to the research in the DOP, but researchers did, nonetheless, entertain certain beliefs about their research, but these beliefs tended to be more scientifically than socially oriented. A prominant belief that was consistently referred to if the members of the DOP were called upon to justify their research was a belief in the ultimately biochemical basis of "schizophrenia". Such a belief was demonstrated to be only one of a range of ill-defined beliefs about schizophrenia that are entertained by researchers across a number of disciplines. This general lack of a consistent and coherent definition of schizophrenia was shown to further support the contention that the context of legitimation of the DOP was not as highly institutionalised as that of the SSP and consequently, that movement between the two contexts was not as noticeable in the DOP (H4). Furthermore, the goals of the researchers in the DOP did not appear subject to any significant changes in relevancy. Although the more general goals of the program were partially established in a context of legitimation they did not become as highly separated from the context of research as did the higher level goals of the SSP (H9).

Given this lower level of institutionalisation of the context of legitimation in the DOP it might well be expected that conflicts arising from movement between contexts of research and legitimation would be less likely. And indeed, conflicts of a similar intensity to those encountered in the SSP were not observed (H5).

The material presented in this chapter has served to provide a basis for comparison between the SSP and the DOP, but as mentioned in the introduction to this chapter, comparison has not been the only function of this chapter. The case study presented has an internal consistency that enables it to stand apart as a separate work, but in one important respect it was never intended to do so - that is, one of the most important functions of this last case study has been the further validation of the theoretical concepts and methodology that were developed in all the preceding chapters.

A more highly systematic and quantitative comparative summary of the empirical material follows in the concluding chapter.

FOOTNOTES TO CHAPTER 7

1. Dopamine and octopamine are metabolites of the organic molecule ℓ-dopa, so that if ℓ-dopa is consumed the blood concentrations of dopamine and octopamine are increased. When it was first discovered by modern medicine in 1967, ℓ-dopa was hailed as a miracle drug because of its potency in the treatment of Parkinson's disease and sleepy sickness (encephalitis lethargica). Enthusiasm has subsequently waned following the discovery of relapses and side effects, but the drug is still a popular one [see Oliver Sacks, Awakenings, Harmondsworth: Penguin, 1976, for a particularly inspired account of the ℓ-dopa story].

2. It is doubtful whether accurate figures are available for the amount of money spent on pharmaceutical research. The main reasons for this appear to be the difficulties of defining "drug research" and the difficulties associated with obtaining accurate economic accounts from the companies responsible for much of the research. According to Klass [1975:72, 73], even the amount of money involved in the sales of the products of drug manufacturing is impossible to calculate.

> "This is due to the fact that the major firms are multinational, operating in countries where different laws apply regarding disclosures required from commercial firms . . . In addition to the difficulty of accurately estimating the money value of drug sales, there is the problem of the definition of "drugs". Drugs for human consumption, drugs for veterinary use, drugs sold over-the-counter on doctor's prescriptions, drugs used as cosmetics, deodorants, herbicides, defoliants, drugs stockpiled for chemical warfare, certain chemicals used as food additives, some drugs freely sold in one country that are illicit in another: all these factors prevent the gathering of accurate figures regarding the total sale of drugs for human medical use . . . Nevertheless the sales of drug companies has been reasonably estimated . . . The total turnover of drug sales in 1971 has been estimated conservatively as sixteen billion dollars, with the leader of the pack a Swiss firm, Hoffman La Roche, doing a turnover in 1971 of one thousand and two hundred and fifty million dollars".

(This is still however, small beer compared to the oil multinationals. In 1973 Exxon earned more money than any other company in history. Net profits alone were in that year,

$2,440 millions).

According to the Australian Pharmaceutical Manufacturers' Association, research and development expenditure was rising steeply during the time span considered for the DOP. In the United States, for example, research and development expenditure by pharmaceutical companies amounted to $US549 million; it rose to $US814 million in 1973, and approximately $US932 million in 1974, or 11.7% of United States sales revenue [APMA Fact Book, 1977, p.38].

3. One of my respondents related how the Roche company has established an extremely high calibre research centre in the United States which is symbolically separated from the company's production end by a barbed wire fence. This fence serves to remind the researchers that they are there to perform curiosity-motivated basic research despite the researchers' frequent inclinations to perform research that is more immediately relevant to the needs of the company. "Research scientists are like most other people", he went on to say, "they like to feel useful". The reason behind this apparently bizarre idea of the drug company's is that the one commodity that is in short supply in the drug industry is new ideas. That means, to take a slightly cynical view, that new ideas and products with novel physiological and psychological effects may be useful in the treatment of established medical problems, or they may actually alert us to unknown medical problems (in other words, health care can be considered like any other industry as needing to expand its markets so as to remain competitive with other types of industry).

4. According to the APMA, "There is a certain minimum size requirement for a company to effectively undertake research . . . It is unreasonable to expect a high degree of large scale human-use research and development to take place in Australia." That is, although Australian sales do contribute to the research and development expenditure made by parent companies since company research budgets are based on global turnover figures - Australian sales of prescription medicines account for only 0.4% of total world-wide sales. Despite these factors, there is a growing tendency for international companies to commence research activities in this country as business develops. [APMA Fact Book, 1977, pp. 39, 40; cf. also Pharmaceutical Manufacturing Inquiry, Canberra: Australian Government Publishing Service, 1979, p.55].

5. For example, the measurement of drug levels in blood samples (which are collected by the department), clinical "rounds", and combined staff seminars. In addition, the department head also worked as a medical doctor, although this was not a formal requirement. There is a sense in which these services to a major hospital were in exchange for the further legitimation of a relatively new type of medically useful discipline.

6. Mullins [1972] refers to the importance of the charismatic qualities of Delbruch to the solidarity of the phage group; it seems reasonable to conjecture that just as with the emergence of religious movements, charismatic leaders may be an important factor in preserving the cohesion of a marginal research program or specialty.

7. Thus, when I spoke to clinical pharmacologists in other universities about the DOP, a degree of scepticism about the dopamine/octopamine hypothesis was usually expressed. The program was not entirely written off, but the potential of the program was questioned. This is reflected in the literature at the time of the establishment of the program. For example, Byck [1973:154] lists the major criticisms:

> "Reported abnormalities in brain chemistry such as the alleged deficiency of dopamine -hydroxylase in the brains of schizophrenic subjects are suspect because of many methodological difficulties, but this field of research may prove fruitful, particularly in view of the success in Parkinson's disease . . . two requirements for identification of the patho-physiological substrate of schizophrenia are that all antipsychotic drugs possess a common specific action and that this specific action can be related to an abnormal characteristic found in the CNS of schizophrenics. The concept of a dopamine-receptor abnormality in particular does not meet the second requirement. Since there are no animals models for schizophrenia, this research path will continue to be difficult. The possibility that any common action of antipsychotic drugs has neurophysiological effects two or three steps removed from the presumed defect in schizophrenia is another difficulty".

8. The main features of a medical model of health is that it is mechanical, reductionist and, ironically, focusses on disease rather than health. In this model the human body is treated

as a complex but mechanically linked system of parts. This approach has of course been one of the main reasons for the spectacular successes of modern medicine, however it may well be a major impediment to further progress. In general, modern medicine is still very much a black art even though it is largely science based.

9. "Scientism" is considered to be a general cultural ideology, but one that is specifically tied to knowledge in the modern physical sciences. The main features of a scientistic approach to nature, self and society are physical reductionism, a mechanical tendency, a concern with objectivity at the expense of subjectivity, the presence of domination and control as a motive to scientific thought and action, and totalism, that is, the assumption that scientism can account for everything. As ideology, scientism is associated with a particular mode of production: capitalism, particularly the post-industrial revolution form of capitalism, since only then did the association of science with industry become a major factor in the institutionalisation of science, forcing, for example, a division between basic research and practice oriented research.

There are, of course, many aspects to the process of separation between scientific knowledge, creative human labour, and human interests that has come to fulfilment since the Industrial Revolution. Particularly important is the way that, as a mode of theoretical production, modern science has inherited a belief in the autonomy of the process and products of science from other aspects of society. This topic has been discussed at greater length in Chapters 2, 3 and 4.

10. The theory and treatment of mental illness appears to reflect prevailing cosmologies. For example, the Egyptians apparently made no connection between behaviour and the brain and relied on the gods to help the "possessed". Earlier cultures however, appear to have linked the brain with mental illness: archaeological remnants indicate that brain surgery (probably just the relieving of pressure from tumours by opening the skull) has been practised for a long time. According to McClure [1973] theories about the brain tended, historically, to fall into three main categories: the brain either accumulated excess heat, pressures of various sorts, or spirits. Theory started to become more reductionist by the early twentieth century; for example, the great Spanish anatomist Ramon yCahal actually spent a portion of his scientific life looking "in the flower garden of the gray matter for the butterflies of the soul" [McClure, 1973:153] thinking that he might find nerve cells with strange and

delicate shapes which might be associated with the cause of mental illness. This theory has not remained the basis of any accepted model but the general idea persists in a different form: the modern butterflies of the mind have chemical forms.

The origins of chemotherapy, which is the medical orientation of the dopamine/octopamine program, are ancient. As early as 2500 BC a Hindustani document called the Rig Veda described a number of medicinal plants. Later Indian documents amplified on the use of these plants. The Charka Samhita, written about 1600 BC listed over 2000 remedies for different medical problems, almost all of which rely on drugs of plant origin. Whilst the use of plants, or the simple derivatives of plants remains largely a part of "folk" medical practices, or the practices of "alternative" healing, the practice of chemotherapy in "advanced", industrial nations has become largely based on the use of complex, *synthetic* drugs.

11. R.D. Laing and D.G. Cooper are famous for their emphasis on the entirely social nature of schizophrenia. In these entirely social theories, the family is considered to be the root cause of psychosis. See for example, Laing's The Divided Self and Cooper's The Death of the Family. For critiques of Laing and Cooper's work see, for example, Boyers and Orril [1972], Clare [1976], and Szasz [1976]. As remarked in Section 4.7 though, the work of Gregory Bateson on the social origins of schizophrenia considerably pre-dates that of Laing and Cooper.

12. Whilst Sedgewick has relied on a specifically Kuhnian model for his analysis, a stance which could not be supported by the recent literature on the sociology of science that has informed this thesis, his general comments are still useful.

13. Although this is a fertile area of research and not without a turnover of different ideas. Another more recent hupothesis as to the most relevant transmitters involved is that prostaglandins, a group of acidic lipids, are the "missing link" - see Horrobin [1980].

14. See, for example, R. Boyers and R. Orril, Laing and Anti-Psychiatry, Harmondsworth: Penguin, 1972.

15. Nonetheless, although this link may have been serendipitous from the viewpoint of the members of the DOP, there does appear to be some precedents in the literature. For example, Barbeau and McDowell [1969], Yarya-Tobias et.al. [1970] and Snyder [1970].

16. In addition to his research in the DOP, the program leader also conducted medical research on other relatively unrelated research programs (viz: aspects of drug binding, metabolism and clinical evaluation). This was in addition to his work in the roles of medical doctor, professor and teacher.

17. Event 14 in Figure 7.6-1, "the serendipitous discovery in 1952 of the selective nature of gold blacks was brought to attention by a colleague" is not counted as a serendipitous event since the serendipitous event was in 1952 rather than being actually part of the events of the program.

CHAPTER 8: SUMMARY OF DATA AND COMPARISONS

> "To put it simply, each science taken singly is impressive; all sciences put together make for a sorry cosmology".
>
> Thomas Luckmann, "Philosophy, Social Sciences and Everyday Life", in Thomas Luckmann (ed.), Phenomenology and Sociology, Penguin, Harmondsworth: 1978, p.219.

8.1 Introduction

In this chapter the various figures, tables and qualitative discussions presented in the two case studies have been reduced to a quantitative basis with the goals of summarising the analyses of the DOP and SSP and facilitating comparisons between them.

The general strategy that has been employed in this chapter is the same as that employed in the individual case studies - that is, the two research programs have been evaluated in terms of the same hypotheses, with the addition of an assessment of the hypotheses which were introduced in Section 4.9 as being more specifically oriented towards comparison. The significant difference in this chapter is however, that the evaluation has been attempted on primarily quantitative basis. This provides some test of the validity of the conclusions reached in the last two chapters, but only a partial test since the same sources of information are being used and often the same interpretive basis has been used to assess the data. The fact that the information presented in this chapter has been transformed through procedures of measurement and counting does however provide a different perspective. Some general remarks comparing the two approaches will be made in the conclusions to this chapter.

Where the variables compared have an obvious basis for quantification the analytical task of this chapter has been relatively straightforward. At times, however, it has been necessary to make judgements based on broad impressions that were not easily quantified - for example, the relative levels of academic status of the programs, and the relative levels of practice orientation of the research. These more sweeping generalisations are not considered to be incompatible with the more easily derived quantifications, since after all, these "easy" quantifications were only able to be obtained on the basis of prior generalisations that were made at an earlier stage in the analysis. The incompatibility, if any, derives then from the stage at which generalisations have been made rather than from the incommensurability of quantitative and qualitative data.

8.2 Tables and figures referred to in this chapter

Much of the data that will be tabled in this chapter has been derived from Tables and Figures that were presented in the last two chapters. For convenience these sources have been listed below in Table 8.2-1.

<u>TABLE 8.2-1</u>: Tables (T) and figures (F) referred to in this chapter.

8.3 Quantitative summary of the SSP and the DOP

A quantified summary of much of the data contained in the two case studies follows in the form of a series of tables. Where possible the sources of data listed in the tables has been referred back to the figures and tables listed in Table 8.2-1. The information contained in the following tables will be used as the basis for a detailed comparison of the SSP and the DOP in the next section.

On the basis of the information contained in the following tables a comparison will however be made in this chapter between the overall levels of institutionalisation of the two programs. This comparison follows in Section 8.3-3.

8.3-1 Orientation of research

(i) TABLE 8.3-1: Professional orientation of cognitive field (constellations of goals and theoretical landscapes) - from T6.7-1, T6.7-2, T7.5-1 and T7.5-2.

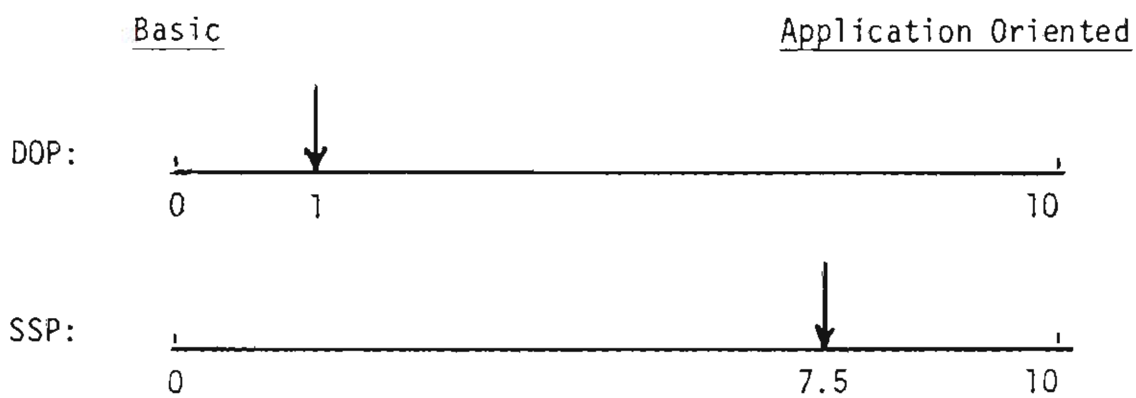
Components of cognitive fields	% of components oriented towards particular professional orientational reference groups*	
	Scientific	Medical
DOP:		
Constellation of goals	83	17
Higher level goals	44	56
More technical goals	93	7
Theoretical landscape	84	16
	Scientific	Engineering
SSP:		
Constellation of goals	48	52
Higher level goals	70	30
More technical goals	44	56
Theoretical landscape	81	19

* Percentages have been calculated on the basis of a score of 1 given to each component (which are particular goals and bodies of theory and which constituted the cognitive structures of the research programs) directly oriented to a particular professional orientational reference group and a score of 1/2 given to both directions in cases where components are directed towards both reference groups [see Tables 6.7-1, 6.7-2, 7.5-1 and 7.5-2].

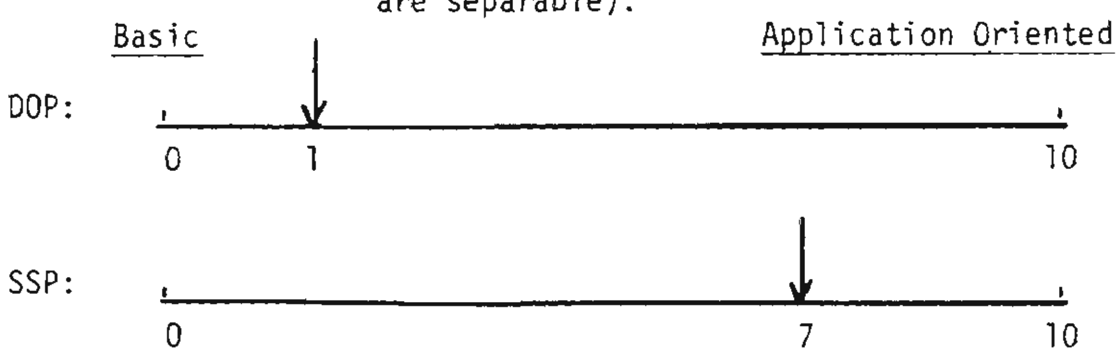
(ii) TABLE 8.3-2: Goal orientation of publications - from Appendices 8 and 16.

	Program	
	DOP	SSP
(a) % of publications directed towards one or other of the program's higher level goals	25	11
(b) Concentration of publications on technical goals	55% of publications on 14% of technical goals	100% of publications on 21% of technical goals

(iii) FIGURE 8.3-1: Scientists' self evaluations of the overall orientation of their research programs.



(iv) FIGURE 8.3-2: Orientation of research on the basis of a general assessment of the contexts of research and legitimation (so far as they are separable).



8.3-2 Indicators of the levels of institutionalisation of the two programs

(i) TABLE 8.3-3: Flow diagram comparisons

Variables	Program	
	SSP	DOP
(a) Time elapsed before the co-ordination of all program members on a particular event - from F6.8-1 and F7.6-1	39 months	3 months
(b) Direction of influence according to type of individuals exerting maximum influence on higher level and technical goals - from F6.9-2 and F7.7-2		
A. Higher level goals	Authoritative non-members	Program leader
B. More technical goals	Various program members	Various program members
(c) Average priority (on a 10 point scale) of core groups for		
A. Higher level goals	5.0	2.7
B. More technical goals	3.8	1.8
- from F6.9-3 and F7.7-3		
(d) Average levels of achievement of goals at the cut-off points of the in-depth analyses of the two progams - from F6.9-1 and F7.7-1		
A. Higher level goals	9.3	4.6
B. More technical goals	6.6	5.2

TABLE 8.3-3 (cont.)

Variables	Program	
	SSP	DOP
(e) Average rate of progress (based on 10 point scale) towards		
A. Higher level goals	5×10^{-2} units/month	7×10^{-2}
B. More technical goals	5×10^{-2}	19×10^{-2}
- from Appendices 1 and 11		
(f) Number of significant serendipitous events - from T6.8-1 and F7.6-1	1(E14)	5(E5, 11, 12, 17, 19)
(g) Node analysis - from F6.8-1 and F7.6-1		
A. (Total number of outputs)/(Total number of inputs)	0.94	1.06
B. Maximum number of inputs to any event (i.e. most "highly focussed" event)	7(E9)	3(E6)
C. Maximum number of outputs from any events (i.e. most "fruitful" event)	3(E9)	3(E12)

(ii) TABLE 8.3-4: Cognitive structure comparisons.

Variable	Program	
	SSP	DOP
(a) Dimensional comparisons of cognitive structures		
A. Number of components in constellation of goals - from T6.7-2 and T7.5-2	24	22
B. Number of components in theoretical landscape - T6.7-1 and T7.5-1	13	13
(b) Professional orientation of cognitive structures - high, medium or low on the basis of information in Table 8.3-1		
A. Overall orientation of components of the cognitive landscape of the research programs		
i. Orientation towards a scientific professional orientational reference group of the:		
Constellation of goals	M	H
Theoretical landscape	H	H
ii. Orientation towards a medical or engineering professional orientational reference group of the:		
Constellation of goals	L	M
Theoretical landscape	L	L

TABLE 8.3-4 (cont.)

Variable	Program	
	SSP	DOP
B. Level of effect of professional orientational reference groups on the components of the cognitive landscapes of the research programs		
i. Orientation towards a scientific professional orientational reference group of the:		
Higher level goals	M	M
More technical goals	M	H
ii. Orientation towards a medical or engineering professional orientational reference group of the:		
Higher level goals	L	M
More technical goals	M	L
(c) Stability of cognitive structures - high, medium or low	H	H
(d) Degree of articulation of belief system - high, medium or low	H	L
(e) Degree of separation of context of research and context of legitimation - high, medium or low	H	L

(iii) TABLE 8.3-5: Autonomy comparisons.

Variable	Program	
	SSP	DOP
Average of autonomy indices* for		
A. Higher level goals	-3.0	0.4
B. More technical goals	-0.5	1.2
- from F6.9-4 and F7.7-4		

(iv) TABLE 8.3-6: Marginality comparisons.

Variable	Program	
	SSP	DOP
(a) Level of academic status	L-M	M
(b) Level of funding	H	L-M
(c) Security of funding	L-M	L
(d) Security of staffing	M	L
(e) Degree of integration of research with science based industries	M	L
- on a five point low, low-medium, medium, medium-high scale		

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* As detailed in Appendix 1, the numerical values of the autonomy index are defined over a continuum of level of influence of individuals over self and others in the research group. The two extremes defined are (a) the situation where the individual is able to exert influence over the goals of others in the group (positive end); (b) the situation where the goals of the individual are influenced by others in the group or outside the group (negative end). A value approaching zero indicates a position of relative individual autonomy.

8.3-3 Comparisons of levels of cognitive and social institutionalisation

The comparisons between the two research programs contained in Table 8.3-7 in this section have been made on a high-low basis. That is, the comparisons are only relative comparisons and do not refer to any established "external" standards about levels of institutionalisation. Thus, where "n.a." (i.e. not applicable) appears in the following tabled information, this means that the levels of the particular variables referred to were approximately the same, a situation which prevents a differential comparison. The sources of the data tabled below are Tables 8.3-3, 8.3-4, 8.3-5 and 8.3-6 as shown by the numbered references in the column of variables.

TABLE 8.3-7: Levels of cognitive and social institutionalisation of the two research programs by various indicators.

Variable	Level of institutionalisation indicated for the research program	
	SSP	DOP
Cognitive in type:		
T8.3-3(f): Number of significant serendipitous events	H	L
T8.3-3(g): Node analysis - A. (Total number of outputs)/(Total number of inputs)	n.a.	n.a.
B. Maximum number of inputs to any event	H	L
C. Maximum number of outputs from any event	n.a.	n.a.

TABLE 8.3-7 (cont.)

Variable	Level of institutionalisation indicated for the research program	
	SSP	DOP
T8.3-4(a): Dimensional comparisons of cognitive structures -		
A. Number of components in constellation of goals	n.a.	n.a.
B. Number of components in theoretical landscape	n.a.	n.a.
Average Levels	H	L
Cognitive and social in type:		
T8.3-3(a): Time elapsed before the co-ordination of program members on a particular event	L	H
T8.3-3(B): Direction of influence according to type of individuals exerting maximum influence on higher level and technical goals		
A. Higher level goals	H	L
B. More technical goals	n.a.	n.a.
T8.3-3(c): Average priority of core groups for		
A. Higher level goals	H	L
B. More technical goals	H	L
T8.3-3(d): Average levels of achievement of goals at the cut-off points of the in-depth analyses of the two programs		
A. Higher level goals	H	L
B. More technical goals	H	L
T8.3-3(e): Average rate of progress towards		
A. Higher level goals	H	L
B. More technical goals	L	H

TABLE 8.3-7 (cont.)

Variable	Level of institutionalisation indicated for the research program	
	SSP	DOP
T8.3-4(c): Stability of cognitive structures	n.a.	n.a.
T8.3-4(d): Degree of articulation of belief system	H	L
T8.3-6(a): Level of academic status	L	H
Average Levels	H	L
Social in type:		
T8.3-5: Average autonomy indices for		
A. Higher level goals	H*	L
B. More technical goals	H	L
T8.3-6(b): Level of funding	H	L
(c): Security of funding	H	L
(d): Security of staffing	H	L
(e): Degree of integration of research with science based industries	H	L
Average Levels	H	L

* On the basis of a relative lack of autonomy being equivalent to a high level of institutionalisation and vice versa.

In Table 8.3-7 the variables that have been used as indicators of levels of institutionalisation of the two programs have been divided into (a) those of a cognitive type, (b) those of a cognitive and social type, and (c) those of a social type (as per Table 5.2-1). Even given that the separation of variables into cognitive and social types does not imply that the cognitive is not in some ways also social and that the social is not also in some ways cognitive (and that the "cognitive and social" category does not fully account for those possibilities) the data are unambiguous about the relative levels of cognitive and social institutionalisation of the two research programs - the SSP has, compared to the DOP, a relatively higher level of cognitive and social institutionalisation.

8.4 Comparison of the research programs

On the basis of the quantitative information presented in the last section a comparison of the SSP and the DOP can be made. This comparison, which follows in Table 8.4-1, has been made through an evaluation of the hypotheses listed in Section 4.9. These hypotheses have for convenience been re-listed in the Table.

The general hypotheses (H1 - H12) have been evaluated in terms of the range of support given to the hypotheses by the data in Section 8.3. The hypothesis which were more specifically oriented towards comparing the research programs (H13-H18) have been evaluated graphically using two charts and four heuristic contingency tables. These diagrams will be presented and discussed in the following section.

TABLE 8.4-1: Comparison of the SSP and the DOP.

Hypothesis	Relevant variables - as listed in Tables 8.3-2 - 8.3-6	Range of support from the data - positive (+), negative (-) or indeterminate (/)		Overall type of support for hypothesis	Hypothesis
		SSP	DOP		
1	T8.3-4bA, B	+	+	+	Scientists are subject to the social and cognitive control of professionalism which operates through the agency of professional orientational reference groups
	T8.3-6(a)-(e)	+	+		
2	T8.3-2a	+	+	+	Professional orientational reference groups provide a basis for scientist's distinctions between and definitions of, scientific and non-scientific activity
	T8.3-3	+	+		
	T8.3-4(b)A,(b)B,(d),(e)	+	+		
	T8.3-6	+	+		
3	T8.3-2(a)	+	+	+	Scientists tend to bracket social considerations about their research as "external" to the research process
	T8.3-3(c)A, B	-)	/		
	T8.3-3(d)	+) +	/		
	T8.3-3(e)	-)	+		
	T8.3-4(c)	+	+		
	T8.3-4(d),(e)	+	/		
4	T8.3-2(a)	+	+	+	Scientists move in thought and action between two sub-universes of meaning; a context of research and a context of legitimation
	T8.3-3(c)A, B	-)	/		
	T8.3-3(d)	+) +	/		
	T8.3-3(e)	/)	+		
	T8.3-4(d),(e)	+	/		
5	T8.3-3(c),(d),(e)	/	/	+	This movement between sub-universes may engender in scientists a conflict of relevancies
	T8.3-4(b)	+	+		
	T8.3-6(a)-(e)	+	+		
6	T8.3-2(a),(b)	+	+	+	Scientific research occurs in the context of a structured cognitive field which consists of interpenetrating levels: metaphysical, theoretical, subject concern, and technical levels
	T8.3-4(a),(b),(c)	+	+		
	T8.3-4(d)	-	/		
	T8.3-5(a)	+	+		
7	T8.3-2(a),(b)	+	+	+	Cognitive structures in the context of research provide structures of relevance for scientists' research
	T8.3-3(a),(c),(d),(e)	+	+		
	T8.3-3(f)	/	/		
	T8.3-4(b)	+	+		
	T8.3-4(d)	+	/		
	T8.3-6(a)	+	+		

TABLE 8.4-1 (cont.)

Hypothesis	Relevant variables - as listed in Tables 8.3-2 - 8.3-6	Range of support from the data - positive (+), negative (-) or indeterminate (≠): SSP	DOP	Overall type of support for hypothesis	Hypothesis
8	T8.3-2(a),(b) T8.3-3(c),(d),(e) T8.3-4(a)A,(b)B(i), (b)B(ii)	+ + +	+ + +	+	Scientists are directed in their research towards a wide range of goals which span different levels of the cognitive field of a research program
9	T8.3-2(a),(b) T8.3-3(c),(d) T8.3-5(a)	+ + +	+ + +	+	Not all the goals that are perceived by scientists to be relevant to their research remain equally relevant
10	T8.3-4(c)	-	-	-	The research goals of scientists change over time
11	T8.3-2(a),(b) T8.3-3(c) T8.3-3(e) T8.3-3(d)	+) -) + ≠) ≠	+ - + + ≠	+	Scientific research is predominantly instrumental by virtue of being more highly directed towards technical goals and the means for their realisation than towards questions about the value of these goals
12	T8.3-3(a) T8.3-3(c),(e) T8.3-4(c)	≠ + +	+ + +	+	Most scientists perform research as part of a research program which constituted through the collective activities of a group of research workers who share a commitment to particular research practices and techniques, who are directed in their research towards a shared set of goals, and who share, to some extent, a common stock of knowledge
		SSP	DOP		
13	T8.3-1 T8.3-2 T8.3-2a T8.3-2(c) T8.3-4(b),A(i) T8.3-4(b),A(ii)	+ + + ≠ + ≠		+	Scientific research varies in its orientation towards social application
14	T8.3-2(a) T8.3-3(b),(c) T8.3-4(d) T8.3-5(a) T8.3-6(a),(b),(c), (d) T8.3-6(e)	≠ + + + ≠ +		+	"Practice oriented" research is more highly constrained by social, economic and political factors than is "basic research"
15	T8.3-4d T8.3-6(a) T8.3-6([illegible])-(e)	+ ≠ +		+	The level of institutionalisation of the context of legitimation of a research program is positively correlated with the level of scientific marginality of a research program

TABLE 8.4-1 (cont.)

Hypothesis	Relevant variables - as listed in Tables 8.3-2 - 8.3-6	Range of support from the data - positive (+), negative (-) or indeterminate (≠) SSP DOP	Overall type of support for hypothesis	Hypothesis
16	T8.3-2a T8.3-3(c),(d)B T8.3-3(e)B	+ + -	+	The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated with the level of orientation of program members towards the more technical goals of the program
17	T8.3-2(a) T8.3-3(c)A,(d)A, (c)A	+ -	-	The level of institutionalisation of the context of legitimation of a scientific research program is negatively correlated with the level of orientation of program members towards higher level goals
18	F8.3-1 F8.3-2	+ +	+	The level of institutionalisation of the context of legitimation of a scientific research program is positively correlated with the level of practice orientation of research in the program

8.4-1 Deviant cases and their analysis

From Table 8.4-1 it is clear that the general hypotheses (H1-H12) have been supported by the data with only one exception, H10. The support given by the data has mostly been positive, the exceptions being usually indeterminancies rather than negations. It is not necessary to go through the data point by point to justify these conclusions, since simple inspection will usually suffice - the logic is mostly self evident and, furthermore, the broad thrusts of the arguments have already been well developed in the case studies. There are a few notable exceptions however, and in the remainder of this section the interpretations of hypotheses 3, 4, 5, 6, 10 and 11 will be discussed in more detail. These discussions are necessary because the data is not unambiguous and requires further explanation.

H3. Three items of data relating to the SSP researchers' priorities and rate of progress towards goals do not appear to support the hypothesis "that scientists tend to bracket social considerations about their research as 'external' to the research process". In the first instance, the average priorities for the higher level goals of the SSP (and the DOP) are higher than the average priorities for the more technical goals (T8.3-3(c)A,B) - given that these higher level goals do express social interests these higher priorities do not support the bracketing hypothesis. A similar argument applies to the average rates of progress towards the goals of the SSP (T8.3-3(e)); this is not the case for the DOP however, since there is, in support of the hypothesis, a significant difference between the rates of progress towards the different level goals (of the order of 300%).

This negative support only follows if the data are considered separately. If the data are considered together, and in the context of data about the levels of achievement of the goals, a different interpretation is possible. What this data may be expressing is merely the mechanics of the process of bracketing. As discussed in Chapter 6, the high level goals of the SSP actually ceased to remain highly relevant to research early in the program's history, by virtue of having been nearly "fully achieved" quite early along. High level goals which have relatively vague criteria of evaluation may be more easily considered by researchers as having high priorities and high rates of progress, particularly if it is politically and practically expedient to do so. Thus, for example, not to argue that a year or two of research by several scientists in the area of solar energy related solid state physics had not established a viable research front (G3) would be very dangerous practice, even at the expense of honesty (not that this was the case in the SSP). Furthermore, what constitutes a viable research front? Without going into details, the criteria *are* vague. What is important evidence for the general analysis however, is not so much the judgement that it may be difficult to assess scientists' evaluations, but the basic data of the scientists' expressed opinions (given that deliberate deception is not in evidence, as it appeared not to be). After all, the hypothesis about bracketing has most meaning precisely at the subjective expressive level.

Much of the data about the DOP that is relevant to H3 is indeterminant. On balance however, the same forces appear to be in operation except that in the DOP the extent of orientation towards social factors in any context is generally lower than in the SSP. Conservatively speaking, the data does not at least contradict the hypothesis.

H4. A similar line of argument applies to data T8.3-3(c)A, B and (e) if it is used to evaluate H4. That is, an apparently high level of orientation towards social factors (as present in high level goals) in the context of research is no evidence for movement between contexts. It only becomes evidence if it is realised that these general goals were felt to be achieved fairly early enabling attention to be more fully directed towards the more technical goals. These high levels of progress and achievement indicate that a separation has occurred - that is, this data is only conceivably the institutionalised "effects" of a movement between contexts. All the data relevant to this hypothesis does have an understanding post hoc quality insofar as it is difficult to conceive what would constitute more direct evidence for actual movement between contexts. Certainly though, more in depth interaction with the scientists and a much more extensive program of observation of the day-to-day activities of the scientists would have provided more information.

H5. The fact that by various indicators a separation had occurred between higher level goals and more technical goals (T8.3-3(c),(d), and (e)) does not necessarily entail an associated conflict of relevancies for the researchers. On the other hand the different degrees of orientation towards various professional orientational reference groups (T8.3-4(b)), particularly in the case of the SSP does not imply that such a conflict was absent. The strength of this last data is more simply, for both programs, that different orientations do actually impinge on the program - the precise context is not so important. The fact of these different orientations, when considered in the light of the relative marginality of both programs (T8.3-6(a)-(e)), does however indicate a real tension for the researchers - economic survival was dependent on balancing different professional demands, particularly in the case of the SSP. It will be recalled that it was argued in Chapter 4 that the more internally knowledge focussed demands of science do generally exist in tension with the more externally directed, client oriented demands of medicine and engineering. This tension was less apparent in the DOP which was more exclusively basic research than the SSP. Not that a medical orientation was absent from the DOP - as indicated by the different orientations depicted in the constellation of goals and theoretical landscape.

The evidence presented in support of a conflict of relevancies is indirect and far from conclusive. Such a hypothesis can only be fully evaluated on the basis of evidence from the "expressive level" of meaning of the research programs - some such evidence was presented in Chapters 6 and 7.

H6. A number of cognitive structures, including theoretical landscapes, constellations of goals and a Solar Energy Belief System (SEBS) have been identified in the case studies and related to processes of research. The various indicators listed in Table 8.4-1 demonstrate through analyses of these cognitive structures, that the cognitive field of the research programs was stratified into theoretical, subject concern and technical levels. Whether or not these levels actually interpenetrate (as per hypothesis) is not however, demonstrated by the data - although an interpenetration between levels and across structures was argued in general terms in the case studies. The existence of the SEBS certainly indicates that the metaphysical level of research exists in some separation from other levels, but it does not follow from this that the context of research has no metaphysical assumptions. In general, the data presented in this thesis is not fully adequate for an investigation of the extent of interpenetration of the different levels. These levels are *assumed* to be interpenetrating to some extent - on the basis of a fundamental postulate that scientific knowledge is context dependent (consistent with a sociology of knowledge approach to the natural sciences).

H10. The high stability of the cognitive structures (T8.3-4(c)) is considered to be negative support for the hypothesis that the goals of scientists change over time insofar as a highly stable structure means highly restricted change within those structures. For example, as discussed in the case studies, the goals of the scientists did not change once they were established. The fact that established goals did not change does not, of course, imply that a structure of goals must remain invariant. In the first instance new goals were established. That is, the goals of scientists did change over time but in the lifetime of the two research programs these changes occurred more as a process of sedimentation of new goals into established structures than by the replacement of old goals. In addition, the periods of observation of the two programs were not sufficiently long to take in the decline or death of a program. Thus, although periods of relative stability of research were observed, this does not imply that such a situation would always prevail. That is to say, if the program had been observed over a longer period of time major goal change might have occurred due to the winding down or re-routing of the program (for whatever reasons).

H11. The higher priorities in both programs of the researchers for the more general goals (T8.3-3(c)) does not support the hypothesis that scientific research is predominantly instrumental. Nor, however, does it negate the hypothesis for, as argued above for H3 and H4, the more general goals of the SSP were bracketed out of the context of research with the effect that research actually became more instrumental over time. By this logic the data presented for the DOP must then remain as negative support for the hypothesis since goal displacement was not in evidence in that program of research. The balance of the evidence presented in the table does however, support the hypothesis.

8.4-2 Discussion of the hypotheses more specifically directed towards comparisons between the two programs*

Table 8.4-1 demonstrates the extent to which the orientation towards application varied in the two programs. The lines drawn

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* Note: All the data contained in Section 8.3 has been transformed in this section to a five point high, high-medium, medium, medium-low, low scale to facilitate comparisons.

between the two program boxes join data pairs corresponding to the quantitative levels of the indicators listed in the figures and tables presented in Section 8.3. The generally positive slpoe of the lines in the diagram demonstrates that the SSP had a higher orientation towards application than did the DOP. That is, H13, that scientific research varies in its orientation towards application is positively supported by the data. If the goals of the SSP are inspected [see Table 6.7-2] it is quite clear that the more general goals, whilst expressing the various social and political interests associated with the processes of their formation, are not more oriented towards social application for that fact. The opposite can be argued - the more technical goals of the SSP are in fact, more immediately oriented towards social application. Compare, for example, the high level goal of developing expertise in solid state physics (G3) with the more technical goal of developing a long sputtering chamber (G14.1). Thus, although the technical goals in some ways depends on the higher level goal (the development of the sputtering process required a knowledge of solid state physics) the technical goal expresses a more immediate interest in the social application of the research (a long sputtering chamber is a pre-requisite for the mass production of the tubular coll-ectors). The more general goals are more strongly oriented though, to political considerations at the disciplinary specialty and program levels of research. At this level the interests of the research program are, of course, not the only interests expressed - those of the Physics School and the University also being strongly expressed.

On this basis the levels of orientation towards the technical goals (and not the high level goals) have been taken as indicating the level of application orientation in Table 8.4-1. This interpretation is not unambiguous however, since it has also been argued that the separation of the SEBS from the context of research of the SSP indicated that researchers tended to bracket social considerations. This is nonetheless, still considered to be the case - the process of bracketing implies the instrumentalisation of the social and economic more than the total exclusion of such factors. As argued in Chapter 6 for example, the overall consequence of the mode of institutionalisation of the SSP was that the orientation towards application of the research proceeded on a relatively narrow social and economic basis. Research proceeded on the basis of considerable bracketing of social issues and became more instrumental over time.

The same argument does not apply to the DOP where high level goals express application orientation more directly than do the more technical goals - compare, for example, G4 (to develop a drug for the control of schizophrenia/psychosis) with G10.1 (to develop a model of the pre- and post-synaptic mechanisms of dopamine and octopamine related neural transmission systems in the brains of molluscs). Thus, for the DOP the relative levels of orientation towards the higher level goals have been taken in Table 8.4-1 as indicating levels of orientation towards application.

Table 8.4-2 gives strong positive support to H14, that "practice oriented" research is more highly constrained by social, economic and political factors than is "basic research". In the table it can

TABLE 8.4-1: A comparison of the different degrees of orientation towards application of the two programs (re. H13)

		SSP	DOP
Orientation towards application by various indicators	L	T8.3-4(b)A(ii)	F8.3-1 F8.3-2 T8.3-4(b)A(i) T8.3-4(b)A(ii)
	L-M	T8.3-4(b)A(i) T8.3-3(c)	T8.3-2(a) T8.3-3(c)
	M		
	M-H	F8.3-1 F8.3-2	
	H	T8.3-2(a)	

TABLE 8.4-2: A comparison of the relative degrees of constraint by social, economic and political factors of the two programs (re. H14)

		SSP	DOP
Relative degrees of constraint by social, political and economic factors	L		F8.3-1 F8.3-2 T8.3-3(b) T8.3-5A T8.3-6(e)
	L-M	T8.3-3(c)B	T8.3-3(c)A,B
	M	T8.3-6(e)	
	M-H	F8.3-1 F8.3-2 T8.3-3(b) T8.3-3(c)A	T8.3-5B
	H	T8.3-5A,B	

be seen that the majority of the lines joining the data have a pronounced positive slope indicating a separation between levels of social, economic and political constraint that is consistent with the hypothesis. Most of the data presented in the Table has already been discussed, but a few points need to be clarified.

The goal orientation of publications (T8.3-2) is undoubtedly a highly politically constrained process. For example, the editorial policies of journals usually constrain the content of articles accepted for publication, or what amounts to the same thing, such policies determine what is not to be published. Whether or not there are more overtly political forces bearing on the publication processes of more application oriented research has however, not been able to be assessed with the data at hand, and so no table entry corresponding to T8.3-2 has been possible.

In Table 8.4-2 no distinction has been made between the social orientation of higher level goals and more technical goals referred to in the entries for T8.3-3(c) in the SSP (the average priorities of researchers for goals). For the sake of comparison with the DOP, both levels are considered to be expressive of social constraint and the different levels in T8.4-2 are simply based on the numerical entries in T8.3-3(c). This decision has been made despite the earlier remarks about the process of bracketing which occurred in the SSP and the way in which the more technical goals of the SSP were favoured over the higher level goals as expressing social interests. Here however, we are concerned with general issues of *constraint* rather than expressed social orientations. It is much harder to judge whether there is more or less social constraint expressed by goals with less expressed social interest - a lower expression of social interest may well occur precisely because of a high level of political constraint to be instrumental and technical rather than more broadly concerned. For that reason no attempt has been made to distinguish between the levels of constraint in evidence in the different levels of goals.

Table 8.4-3 contains three heuristic contingency tables - the data available is not sufficient to justify a more rigorous statistical analysis. The tables are, however, a valuable summary of data and provide positive support for three of the last four comparatively oriented hypotheses and negative support for one of them. As for the last two tables the lines in the table connect pairs of data corresponding to particular variables. The tables do not have a quantitative basis for the judgement about the relative levels of institutionalisation of the contexts of legitimation of the two programs. The relevant criterion here is the presence or absence of an articulated and coherent set of beliefs about research - such as was present in the SSP and absent in the DOP. On that basis the contexts of legitimation of the two programs have been judged as occurring at different ends of the spectrum with a high level and a low level of institutionalisation respectively.

On the basis of the generally negative slopes of the lines in the relevant tables it is clear that hypotheses 15, 16 and 18 have been supported by the data - these slopes indicate a positive correlation between the three sets of variables. That is, the levels of

TABLE 8.4-3: Heuristic contingency tables.

		Level of institutionalisation of the context of legitimation of research program: L	L-M M M-H	H
H15 Level of scientific marginality by various indicators	L	T8.3-6(a) (d),(e)		
	L-M	T8.3-6b		T8.3-6(a), (c)
	M			T8.3-6(d), (e)
	M-H	T8.3-6(a)		
	H			T8.3-6(b)
H16 Level of orientation of scientists towards more technical goals by various indicators	L	T8.3-3(c)B		
	L-M			T8.3-3(c)B (e)B
	M	T8.3-3(d)B		
	M-H	T8.3-2(a)		T8.3-3(d)B
	H	T8.3-3(e)B		T8.3-2(a)
H17 Level of orientation of scientists towards higher level goals by various indicators	L	T8.3-3(e)A		T8.3-2(a)
	L-M	T8.3-2(a) T8.3-3(c)A		T8.3-3(e)A
	M	T8.3-3(d)A		
	M-H			T8.3-3(c)A
	H			T8.3-3(d)A
H18 Level of practice orientation of research	L	F8.3-1 F8.3-2		
	L-M			
	M			
	M-H			F8.3-1 F8.3-2
	H			

institutionalisation of the contexts of legitimation of the research programs were found to be positively correlated with the levels of scientific marginality of the programs (H15); the levels of institutionalisation of the contexts of legitimation of the research programs were found to be positively correlated with the levels of orientation of the program members towards the more technical goals of the programs (H16); and the levels of institutionalisation of the contexts of legitimation of the research programs were found to be positively correlated with the levels of practice orientation in the programs (H18). All of the data presented in these tables has been adequately discussed and these results will not be discussed further. The table corresponding to hypothesis 17 does, on the other hand, present an apparent anomaly and calls for further discussion.

H17. Contrary to expectations the level of institutionalisation of the context of legitimation of the two research programs appears to be positively correlated to the level of orientation of the members of the program towards higher level goals. The major assumption behind the original hypothesis was that the higher level goals of a research program would generally be more oriented towards social factors than the more technical goals. Thus, when the context of legitimation of a research program was highly institutionalised it was assumed that the level of orientation towards social factors in the context of research would be correspondingly reduced, and vice versa in the case of a low level of institutionalisation of the context of research. However, as we found in the discussion of H13, the more technical goals of the SSP were not less socially oriented for their increasingly technical orientation over time (by virtue of the goal of producing a commercially viable end product). The higher level goals of the SSP were however, more narrowly concerned with political-economic interests of the local institution than was the case for the DOP. Thus, these higher level goals were considered by the scientists in the SSP to be relatively more important (by various criteria) than the higher level goals were for the scientists in the DOP - in the latter case the social interests of the scientists tended to be more diffused through the whole constellation of goals.

Hypothesis 17 is no doubt too simplistic. It would seem unlikely that a simple generalisation relating the relative levels of institutionalisation of the contexts of legitimation of research programs and the levels of orientation of scientists towards higher level goals could hold for all research programs - there appears to be far too many factors involved. The negative result calls for more information, that is, more case studies of research programs.

8.5 Conclusions

In general the material discussed in this chapter has supported the analysis contained in Chapters 6 and 7. Given that the data presented in this chapter is to some extent new and differently based, this chapter stands as a partial validation of the analysis in Chapters 6 and 7, although as stressed earlier, there is an obvious overlap of material and so as a test of validity this chapter can be at most, partial.

The analysis in this chapter supports the general assessment that the SSP and the DOP are widely separated with respect to levels of institutionalisation and orientation towards application - the DOP is less highly institutionalised cognitively and socially than the SSP and also less oriented towards social application than the SSP.

The material presented in this chapter has provided evidence to suggest that some revision of the hypotheses developed earlier in this thesis is necessary. These revisions were implicit in the case studies (and in the case of H10 actually discussed), but given the additional analysis contained in this chapter, a strong case for the intimated changes has now been made. The evidence available will not support H6, H10 and H17. That is, regarding H6, there is insufficient data to fully support the (sub)hypothesis that the levels of the cognitive field of the researchers were interpenetrating; regarding H10, change in the goals of scientists was of a restricted nature; and regarding H17, the data did not support a postulated positive correlation between the level of institutionalisation of the context of legitimation of the research programs and the levels of orientation of scientists towards higher level goals. The material presented in this chapter did however, support a revised version of H6 and H10. That is,

H6*: Scientific research occurs in the context of a structured cognitive field which consists of different levels of structure: metaphysical, theoretical, subject concerns and technical levels.

H10*: The research goals of scientists change over time, but in the lifetime of a research program this change occurs more as a process

of sedimentation of new goals into established structures than by the replacement of old goals.
With respect to H17, it was concluded that the data was insufficient to warrant further speculation.

There are some obvious shortcomings in the data and analysis presented in this chapter. Most generally speaking, this chapter is an example of what could be called "over-objectification" - that is to say, without other supporting evidence of a more "expressive" level, hypotheses that obviously depend on the personal assessments of individual scientists cannot be adequately assessed. For example, the hypotheses that dealt with the postulated movement of scientists between different "contexts" (H4), and the postulated conflicts that this movement might engender (H5), were not able to be fully explored with the type of information available - data about the stratification of goals, or the existence or non-existence of a coherent belief system. The adequate evaluation of hypotheses of such a more subjectively oriented nature would require detailed empirical investigations of the constitution of individual consciousnesses - a task that was not attempted in this thesis.

The sense of data that was interpreted in isolation from other related data was questioned on a number of occasions. The meanings of individual pieces of data relevant to H3, 4 and 11 were overturned when considered in the context of other data. One obvious generalisation on the basis of that experience is that more fully contexted data may provide a different picture than data that is treated in relative isolation - a principle that is, after all, often taken for granted in methodological discussions, but rarely demonstrated in research.

Considered as an extension of the two case studies, this chapter clearly adds to the general analysis - particularly at the "objective" level. In many respects however, the analysis requires more information - this is to be expected in any research, of course, since no empirically based analysis can even be properly considered as "finalised". Nonetheless, at this stage of the proceedings we have reached a point of relative finalisation in this analysis. In the final chapter which follows a broad overview of the thesis and general concluding remarks will be presented.

CHAPTER 9: SUMMARY AND CONCLUSIONS

> "But against this project in full realization emerge other projects, and among them those which would change the established one in its totality. It is with reference to such a transcendent project that the criteria for objective historical truth can best be formulated as the criteria of its rationality:
>
> (1) The transcendent project must be in accordance with the real possibilities open at the attained level of the material and intellectual culture.
>
> (2) The transcendent project, in order to falsify the established totality, must demonstrate its own *higher* rationality in the threefold sense that
>
> (a) it offers the prospect of preserving and improving the productive achievements of civilization;
>
> (b) it defines the established totality in its very structure, basic tendencies, and relations;
>
> (c) its realization offers a greater chance for the pacification of existence, within the framework of institutions which offer a

greater chance for the free development
of human needs and faculties."

Herbert Marcuse, One Dimensional Man, Sphere Books, 1970, p.175.

9.1 Introduction

The sociology of science has not so far provided a coherent theoretical framework which can encompass structure and meaning within the cognitive and social processes which constitute science. In an effort to remedy that situation, this thesis contains a structure of related concepts which have provided a basis for detailed empirical investigations of the cognitive and social institutionalisation of two groups of Australian research workers.

The empirical research in the thesis was designed to assess the theory developed in Part 1 of the thesis against a range of different conditions - this theory was broad in scope, covering metaphysical through to technical aspects of the cognitive and social institutionalisation of the natural sciences. Given the normal restrictions of an individually conducted research program, the extent to which such a broadly based theoretical structure could be assessed was limited, but nonetheless, the empirical results have provided encouraging support for the theory and methodology that have been developed.

Despite the restricted nature of the empirical research, the theory remains as a broadly coherent structure which *can* provide the basis for detailed empirical research ranging through microscopic analysis of day-to-day research to more abstract studies of the historical evolution of scientific knowledge. Both these levels of empirical analysis are contained in the thesis, though the thrust of the research was towards the in depth analysis of two small groups of scientists over a very small (historically speaking) period of time - of the order of five years.

The theory developed in this thesis was drawn from a number of different sources, ranging through the sociology, philosophy and history of science and general sociology. The most influential theorist was Alfred Schutz, but his basic insights were augmented by the work of a large number of other scholars including Richard Whitley, Michael Mulkay and Stephen Hill (to name the most significant contribution from within the sociology of science). On the basis of Schutz's broad understanding of social action and the structures of the life world I have been able to develop a general theory of science as a goal directed social process, a perspective which is new to the sociology of science. Nonetheless, the general perspective in this thesis has come to incorporate much material that is decidedly un-Schutzian in its concern with conflicts in the worlds of scientists. This entailed a sympathetic reading of conflict theory ranging through political economy to social psychology. This is not to say that the thesis contains a coherent political economy or social psychology - these perspectives are present, but more as peripheral components of an exploration that has been mostly focussed towards a detailed study of structures in scientific consciousness.

Starting from the observation of an apparent contradiction between a widely disseminated image of the natural sciences as being directed towards socially useful ends and the living reality of scientists who often appear to be conscientiously determined to bracket social concerns and social analysis out of the context of their research, a number of concepts relevant to the understanding of science as an institutionalised and goal directed activity have been developed. This theory and the related empirical case studies are summarised below.

9.2 Summary

9.2-1 Part 1: Theory

Chapter 2: What is science? Some fundamental definitions

In Chapter 2 science was described as both a universe of meaning and a system of theoretical production. These definitions are not mutually exclusive despite the differences in their theoretical traditions - phenomenology and structural-functionalism respectively. Both perspectives are in fact necessary to an analysis which is reflexively aware of different levels of meaning (in Mannheim's sense) and which attempts to integrate these levels - as has been attempted in this thesis. The success of this integration will be discussed shortly.

In Chapter 2 science was also described as professionally mediated. It was argued that the relationship between individual scientists and shared structures of scientific and technical knowledge is affected through the agency of reference groups, but that this relationship is at all times mediated by scientific professionalism - which as Terence Johnson [1973] uses the term, is a form of "collegiate" social control. Professional behaviour implies a range of attitudes and values (as Johnson and many others have discussed), but central to the attributes of professionalism is the value of autonomy. This value sustains the social fabric of science. The internalisation of autonomy as a value is a particularly efficient form of social control - the typical well socialised scientist is able to function as an effective professional without constant direct scrutiny from within the profession. Thus, when the scientist is acting as s/he chooses in relative autonomy, s/he is acting both "naturally" and at the same time professionally - that is, in good accord with the attitudes and values internalised during a typically lengthy period of adult socialisation; consequently, the system of science tends to remain normatively self sustaining.

In this chapter it was argued that the sociology of science has tended to avoid the in depth study of laboratory life and the concrete processes of research. Symptomatic of this situation is the general reluctance of researchers to give adequate attention to that social and cognitive collectivity most immediately relevant to the production of scientific knowledge - the research program, which was defined as that sub-universe of meaning constituted through the collective activities of a group of research and support workers who share a commitment to particular stocks of specialised knowledge, research practices and techniques, and who are directed towards a shared set of goals. This reluctance is most certainly linked to the continued use of theoretical perspectives which do not adequately conceive research as goal directed projects of action.

In addition to the research program, other collectivities were also defined as relevant and real in the processes of research - viz, disciplines, specialties and research areas, as defined by Whitley [1976].

Scientific research is not, it was argued in Chapter 2, a unitary phenomenon, and different types of research were defined. Basic research and practice oriented research are distinguishable as two different types of research defining different ends of a spectrum of application towards orientation - as reflected in the goals of scientists producing knowledge and the socially oriented practical utility that the knowledge is perceived by its users as having.

Scientific research is also distinguishable by its different levels of institutionalisation. However, as discussed in the chapter, the institutionalisation of all research has both cognitive and social aspects, and thus it is possible to distinguish between levels of cognitive institutionalisation of research and different levels of social institutionalisation of research. This distinction between cognitive and social aspects of research is intended as analytical tool which cuts across concepts such as finite province of meaning, sub-universe of meaning and system of production so as to facilitate a confrontation with the traditional concerns of the sociology of knowledge with the relationship between knowledge and social structures. As it was argued, the concept of research as goal directed projects of action provides a link between social structure and the knowledge produced by research. This link which has not so far been adequately conceptualised either in the sociology of knowledge or the sociology of science would seem to be possible only through a deeper understanding of the way knowledge is produced as a consequence of goal directed social action.

The cognitive dimension of scientific research has been neglected in the sociology of science - that is, science has tended to be conceived as a "black box". However, since the work of Kuhn more attention has been given to the constitution of the cognitive fields of scientists. In this chapter it was confirmed that the cognitive fields of scientists tend to be highly structured (as originally suggested by Kuhn, Masterman and others). Metaphysical, theoretical, subject concern and technical levels of structure of the cognitive fields of scientists were distinguished theoretically as stratifications of "the context of research", a context in which scientists are primarily oriented towards research and the production of scientific knowledge. This context is not the only context which is significant as part of the sub-universe of meaning of the research program, however, since modern day scientists also necessarily engage in processes of legitimation of their research. To the extent that scientists internalise a belief in the value-freedom of research and the general irrelevance of social considerations to research it was suggested that a definite separation, in the consciousness and practices of scientists, between these two contexts will exist.

Chapter 3: Images of a directed science

In Chapter 2 research was defined as occurring along a spectrum of different degrees of orientation towards application. Whilst it was a relatively simple theoretical matter to define basic research and practice oriented research as two distinct types of research, occurring at opposite ends of the spectrum of orientation towards application, the kinds of empirical information available for making practical sense of such distinctions are sometimes ambiguous. That is, the images of science that are widely accepted contain certain contradictions. In Chapter 3 it was demonstrated that although the image of a science directed towards socially useful ends still continues to be projected in official statistics, many scientists, particularly members of the scientific "elite", still strongly defend science as being ideally a-political and concerned solely with the a-social pursuit of truths about nature. There are fairly obvious political reasons why scientists should seek to defend this latter image of science - these reasons are primarily related to the need to preserve the autonomy of science against "external" encroachment. Behind all of these relatively prominant concerns the question as to what the ends of science, as it occurs in the labories and studies of practicing scientists, actually are, remains as the key to the often confusing public image of science. In an attempt to untangle the extent to which science can be rightly considered as oriented towards socially useful ends data from the Australian science survey "Project SCORE" was analysed over the period 1968-1974. On the basis of that data (which at the time of writing in 1980 was all that was available) it was concluded that less than 10% of Australian science could be conceivably discussed in isolation from a practically concerned socio-economic structure.

In Chapter 3 it was concluded that the kind of data that has in the past been used to make judgements about the actual orientation of scientific research is lacking in knowledge of the actual goals of practicing scientists. A knowledge of the real goals of scientists (as opposed to politically engineered impressions) does however, presuppose an understanding of what goals, as such, are and what the various processes generally involved in their formation, evolution and achievement might be. This last subject formed the subject for the next chapter, Chapter 4.

Chapter 4: Scientists have goals

Chapter 4 completed the theoretical basis for the case studies which followed in Chapters 6 and 7. In this chapter the general nature of the goals of scientists were explored in detail - viz, the relationships between the goals of scientists and intentionality of consciousness, projects of action, reference groups, and the institutionalisation of research programs was developed. Whilst this chapter was theoretically specialised its basis was firmly in the experienced reality of daily life. This basic appreciation of contemporary daily life is sufficiently important to bear restating in the present context.

Twentieth century life has been enormously affected by the general culture and products of the natural sciences. All sociologists, social philosophers and historians appear to agree on that. That is to say, there is no disputing that the natural sciences have had an effect on modern civilisation - this effect is usually measured in material terms of the way that science based products and expertise have permeated all aspects of modern life. The value of the effects is often debated, of course; the reason for this ongoing debate is that because of the general pervasiveness of science and technology their continued review becomes an integral part of all discussion about progress and change - contentious subjects in any age.

The instrumental rationality of both the natural sciences and everyday life is one aspect of the general "problematic" that tends to be glossed over - probably because instrumentalism is so widely accepted as a necessary aspect of all pragmatically motivated behaviour. Not that the subject has been adequately understood, however. Chapter 4 was intended to reopen discussion about the constitution, in action, of modern day consciousness, and to deepen understanding about the constitution, in action, of scientific consciousness (that is, consciousness as shared by natural scientists).

The natural sciences provide a paradigm case of goal directed behaviour - goal rationality being a dominant form of rationality in modern times. In Chapter 4 the nature of goal directed behaviour in the natural sciences has been explored through an integration of phenomenological, symbolic interactionist and structuralist perspectives. On the basis largely of the work of Alfred Schutz, goals were conceptualised as objectifications of the "in-order-to motive" of action.

The first premise of goal orientation was the intentionality of consciousness - that is, as emphasised in phenomenology all our thoughts refer to objects in consciousness that are, in their essence, meaningful. Thus, goals as objects of consciousness, exist as subject-in-relationship-to-the-object and object-in-relationship-to-the-subject. The social nature of the mode of the intentionality of goals in scientific research formed the subject of the remainder of the chapter.

Scientific research was described in Chapter 4 as projects of action within the framework of a research program, or in other words, the research program was re-defined here in Schutzian terms as the institutionalised form of thematically, motivationally and interpretationally related projects of action. This concept of the project incorporates a concept of goal since a defining feature of action undertaken in programs of research is that such action is to some extent devised in advance. On that basis, goals were defined in phenomenological terms as objectifications of the "in-order-to motive" of action. In those terms, goals are a necessary component of all action. What is not externally necessary however, is the contemporary mode of institutionalisation of the processes of formation, evolution and achievement of goals. Given the apparent reification of many aspects of social life in a capitalist social system it was postulated in the chapter that the goals of scientists may often be reifications rather than freely evolving objectifications of creative human potential.

The concept of professionalism was central to the discussion of the institutionalisation of goal orientation. This concept was developed as a way of accounting for the prevailing system of social control within science. The concept is broadly useful in understanding the processes of institutionalisation of scientific consciousness, but Johnson's concept of "collegiate control", and indeed all his other related concepts, do not provide a fully adequate theoretical basis for an understanding ofprofessional socialisation and professionalised "universe maintenance". Towards that end the concept of "professional orientational reference group" was developed as the major reference group which mediates between individual consciousness and shared resources within science. Through the agency of the professional orientational reference group a structured cognitive field is generated and supported in individual scientific consciousness.

The concept of professional orientational reference group was predicated on the existence of processes of legitimation that are necessary for the preservation of professional identity. In practice most scientists distinguish between their research and the utility and social consequences of their research. This separation reflects a distinction between two sub-universes of meaning within the research program - the context of research as opposed to the context of legitimation. The theoretical basis of these two concepts was further developed in this chapter in terms of their nature as sub-universes of meaning, alternation between which provides tension release - that is, the context of legitimation functions as an institutionalised safety valve. The institutionalisation of these two contexts within a research program may not be sufficient to eliminate all tension however; indeed, such processes of institutionalisation may actually reify already existing conflicts of relevance.

This latter subject was briefly discussed in terms of Gregory Bateson's "double bind theory of schizophrenia".

The institutionalisation of goal orientation was summarised as having three basic features. Scientists are directed in their research by goals which are

1. Established as the result of social and political processes which involve dynamic interaction between interest groups which may involve or exclude direct scientific interests, and which may be directly or indirectly perceived by scientists.

2. Mediated by scientific, social, economic and political considerations and expressed at varying levels of generality; these mediated versions may be expressed within "official" statements of research programs or they may be deeply embedded in the structures of relevance of research.

3. Dynamically linked to an evolving body or bodies of scientific knowledge such that research and the goals of research are only analytically separable; both cognitive and social aspects of research are directed and constrained by orientation to goals which are posited and potentially continually redefinable in terms of changing theory, techniques and social conditions.

On the basis of the theoretical material contained in Part 1 of the thesis, Chapter 4 was concluded by the postulation of fourteen hypotheses. These hypotheses have formed an important basis for the organisation of the empirical material in the two case studies which have been developed as an exploration of the theory in Part 1. The hypotheses were divided into general propositions which cover, potentially, all research in the physical sciences, and propositions specifically oriented towards the comparison of research programs.

9.2-2 Part 2: Methodology and case studies

Chapter 5: Methodology

Chapter 5 is a practically oriented introduction to the empirical research presented in the case studies. The chapter contains a description of the research design of the thesis, the indicators used in the testing of the hypotheses, the techniques used in the fieldwork and some theoretical material in support of particular methodological issues.

In the fieldwork two groups of university based scientists were selected to provide the basis for in depth longitudinal studies of processes and structures involved in scientific research. The scientists chosen as subjects were physicists and neuropharmacologists - although such labels conceal a wide range of disciplinary and specialty interests that were expressed in the research of the scientists. That is, despite the similarities between the two programs (for example, their university context, their size and their recent establishment) there were sufficient differences to enable a fruitful comparison between the two programs. The generation of a comparative basis through the operationalisation of the concepts of levels of cognitive institutionalisation, social institutionalisation, and practice orientation have been important undertakings of this thesis (as outlined in Chapter 5). The empirical material used in the case studies was gathered over a period of approximately three years, between 1976 and 1979.

The major methodological innovation of this thesis has been the development of a "method of repeated feedback". This procedure was primarily designed to increase the accuracy of sociologists' accounts of scientific research. This is an innovation that is particularly necessary in situations where the sociologist may be somewhat unfamiliar with the substance of his/her respondents' research and/or where the existing accounts in the literature do not provide an adequate historical account (which is usually the case since the published literature of physical scientists is notoriously a-historical). In the fieldwork conducted with the two groups of scientists, research accounts were generated through an iterative process which relied on the scientists to check and up-date a series of descriptions of their research. Open ended interviews were initially conducted with a large number of researchers associated with the work of the two groups in order to provide mutual orientation and the necessary information for a generalised "first round" description of the scientists' research, and a questionnaire which sought more specific information. These interviews were also preparatory for continued in depth interaction with the most centrally involved scientists in each of the research programs. The first questionnaire which contained a reconstruction of the scientists' research goals, theoretical landscape, significant research events and social factors important in the establishment of the research goals was then administered to all the scientists who were identified as constituting the research programs. The scientists' reactions, corrections and comments about this material were gathered during a series of follow-up interviews. On the basis of that up-dated information a second round reconstruction and questionnaire was then administered to the "core" scientists. Another series of follow-up interviews with these "core" members provided the "final" syntheses which has been presented in this thesis.

Insofar as the method can be used to prevent unintended discrepancies between sociologists' impressions of scientists' research and scientists' understandings, the method is particularly useful for the generation of accurate research accounts. In addition, since the method relies on the achievement of a consensus between scientists about much of the content of the research account being generated, the method has an in-built test of validity (so far as scientists can be credited with best knowing about the course of their research).

The method is novel in the sociology of science which has to date not generated case studies of the depth required to adequately test much of the theory that has been developed in this thesis. That is to say, the method was developed in response to theoretical needs which had not previously been considered important in the sociology of science, and so it is not altogether surprising that similar techniques have not been previously employed in the field. I should add that the method is not altogether original in that it was inspired by the "Delphi Technique" of consensus achievement which is widely used in other fields of research.

Chapters 6, 7 and 8: Two case studies and comparisons between them

The two case studies in this thesis (Chapters 6 and 7) have been designed to operationalise much of the theory that has been developed in Part 1 of this thesis. In this empirical work the concepts of cognitive and social institutionalisation, cognitive field, cognitive structure, professionalism, and research program have been developed as a general theoretical matrix for in depth analysis of scientific research. In the case studies a number of phenomenologically derived concepts have also been developed in an empirical context - viz, the concepts of finite province of meaning, horizons of meaning, projects of action as goal directed, and types of relevancy. In addition to the development of these basic concepts I have attempted to capture the fragmented mode of the institutionalisation of contemporary science through the concepts of context of research and context of legitimation.

Both the case studies have been used to elucidate, assess and develop the hypotheses that were introduced in Section 4.9, but as stressed throughout the thesis there has been no attempt to design "experiments" to "test" the hypotheses in the way that a natural scientist might deal with physical objects, or a psychologist deal with "experimental subjects". The hypotheses have been developed with the primary aim of fruitfully organising information gained from the relatively naturalistic processes of formal interviewing and informal interactions in scientists' leisure time. For the sake of brevity the summaries which follow have been couched largely in terms of these hypotheses. These summaries have been indexed with reference to hypotheses 1 to 14 (H1 to H14). The original statements of these hypotheses may be found in Section 4.9-1.

The subjects of the first case study were a group of Australian solar energy physicists. Two of the goals that have been important in directing the research of the physicists in what I have termed the "selective surfaces program" (SSP) were firstly, to develop a new and efficient selective surface (which is basically a means of improving the efficiency of solar collection of a surface), and secondly, to develop a commercially viable collector which will employ the new surface. The first goal has been achieved, but the second goal remains as a major direction for research and development - although a promising solar collector has been demonstrated, the commercial viability of the device cannot be known until more work, particularly on the development side, has been completed.

Research on the "selective surfaces program" was found to be highly subject to the authority of a theoretically and technically oriented "cognitive structure". This theoretical orientation was a little surprising given the ostensively strong practice orientation of most of the members of the program. Probably as a consequence of this theoretical orientation, individual program members appeared to be, in the "context of research", relatively uneffected by the impingement of considerable economic, social and political forces on the program.

There was however, a "double bind" situation present in that the researchers felt themselves constrained to be both good physicists and socially useful, the two demands not being particularly compatible.

This double bind appeared to be partially resolved through the separation of a "context of research" from a "context of legitimation". This separation involved the following mechanisms:

(i) the internalisation of socially/politically/economically oriented beliefs about the value of solar energy research in the form of a "solar energy belief system" which was held separate from the actual processes of research and which functioned largely as a means of legitimating a somewhat scientifically marginal area of research; and

(ii) a proliferation and focus on the more theoretically and technically oriented goals of the research program. In this way a greater level of individual control over day-to-day research was possible.

The subjects of the second case study were a group of Australian neuropharmacologists. The research program of these scientists which I have termed the "dopamine/octopamine program" (DOP) was directed towards the general goals of elucidating dopaminergic and octopaminergic mechanisms in the human brain and determining their role in schizophrenia. One of the long term goals of the program was the production of a drug for the relief or cure of schizophrenia.

This second case study, whilst being a self contained analysis, was largely presented to enable comparisons to be made between the two programs. It was found that compared with the SSP, the DOP was not as highly institutionalised, cognitively or socially. Thus, for example, the level of serendipity that was incorporated into research in the DOP was considerably higher than that in the SSP - this indicated that choices had to a lesser extent, been foreclosed by the authority of pre-formed social and cognitive structures. Furthermore, the establishment of the research program followed a path more of gradual evolution incorporating the resources immediately available than the simple adoption of pre-formed research strategies. In addition to this lower level of cognitive institutionalisation, the DOP was constituted in a complex and unstable economic support system. Overall then, the DOP differed significantly along a number of dimensions when contrasted with the SSP.

One of the major points of comparison in this thesis has concerned the relationship between scientists' research and their legitimation of that research. On the basis of the material presented in this thesis the typical "context of research" in the physical sciences has been demonstrated to be a highly institutionalised form of goal directed behaviour. The value of these goals may have been debated before the programs of research were undertaken and the goals of research may have been advocated and defended during the evolution of the programs, but by and large the goals of research did not change once a commitment had been made to them. In the case of the SSP, this stability (/rigidity) in the orientation of research was enhanced by the way in which the values of, and justifications for, a particular piece of research tended to become sedimented in a more or less institutionalised belief system. The exact nature of the relationship between legitimation and research does not however, appear to be capable of a simple or mechanically invariant specification. On the basis of the case material two possibilities emerge. Given a situation where, for whatever reason, a relatively coherent system of beliefs about the purpose and value of research may emerge (for example, in the SSP), this belief system tended to be held apart (in the minds of scientists) from scientific research tasks and tended to emerge in "external" social contexts - that is, in contexts of legitimation.

On the other hand, where beliefs about research were not highly articulated (for example, in the DOP), research and metaphysics are likely to become more closely connected - the "higher level" goals of research in particular may well strongly reflect these beliefs. The "reality" of research will still, in all likelihood appear highly technical - professionalism in science (and the "inward" directed, technical orientation that professionalism in science dictates), is a force that prevails over all of the sub-universes of science.

In terms of the hypotheses that were postulated in Chapter 4, a number of general conclusions were made about both the groups of scientists studied (in the conclusion to Chapter 7):

Both groups of scientists were shown to perform research as part of a research program which was constituted through the collective activities of a group of research workers who shared a commitment to particular research practices and techniques, who were directed in their research towards a shared set of goals, and who shared, to some extent, a common stock of knowledge (H12).

The researchers were subject to the social and cognitive controls of professionalism which operated through the agency of professional orientational reference groups. In the SSP these reference groups were science and engineering, and in the DOP the reference groups were science and medicine (H1). These reference groups provided a basis for the scientists' distinctions between, and definitions of scientific and non-scientific activity. Research in the DOP was, in contrast to the SSP, more highly directed towards a scientific professional orientational reference group which provided the scientists with relatively non-social criteria for their basic research (H2). Like the researchers in the SSP, the members of the DOP did, however, tend to quite generally bracket social considerations about their research as "external" to the research process (H3).

Research in both programs occurred in the context of structured cognitive fields which consisted of theoretical, subject concern and technical levels. Two structures were identified in both programs: a theoretical landscape and a constellation of goals. These structures were stratified into disciplinary, sub-disciplinary, and program levels of research (H6). The two structures provided structures of relevance for scientists' research (H7). It was argued that these structures provided thematic, motivational and interpretational relevancies for research.

The members of both research programs were directed towards a variety of goals which occurred at different levels of the cognitive fields of the research programs (H8). These constellations of goals were stable, cumulative structures, which evolved in conjunction with research by a process of sedimentation of new goals (H10).

Research in both programs became increasingly technical in orientation as the programs evolved (H11).

Although both programs were shown to be in many respects similar, a number of major differences between the two programs have also been demonstrated. These differences have been described in the conclusions to Chapter 8, and in Chapter 9 which was explicitly concerned with a comparison of the two programs as the basis of quantitative data that was abstracted from the empirical material presented in the case studies. In summary:

The DOP was, by virtue of its very basic orientation, not nearly as oriented towards social application as the SSP (H13). Research in the DOP, was as a consequence, far less constrained by considerations of the economic feasibility of research products - although researchers were nonetheless constrained by social factors as analysis of aspects of the process of formation and evolution of the research goals revealed (H14). Research in the SSP was eventually much more heavily funded than research in the DOP, and the more highly constrained contract nature of this funding provided greater motivation for the achievement of practical results.

The context of legitimation of the DOP was less highly institutionalised than that of the SSP. In the SSP a coherent set of beliefs, the "Solar Energy Belief System" was described as being institutionalised in the context of legitimation of most solar energy researchers. There was no clearly articulated belief system relevant to research in the DOP, but researchers did nonetheless entertain certain beliefs about their research - these beliefs tended to be more scientifically than socially oriented. The most prominant, but nonetheless ill-defined belief of the members of the DOP was a belief in the ultimately bio-chemical basis of "schizophrenia". This belief was consistently referred to if members of the DOP were called upon to justify their research. This belief was demonstrated to be only one of a range of ill-defined beliefs about schizophrenia that were entertained by researchers across a number of disciplines. This general lack of a consistent and coherent definition of schizophrenia was shown to further support the contention that the context of legitimation of the DOP was not as highly institutionalised as that of the SSP and that consequently movement between the two contexts was not as marked in the DOP (H4). Associated with the differences in institutionalisation of the two contexts of legitimation of the two programs, a difference in the levels of conflict in the researchers associated with movements between the two contexts was also apparent. That is, the members of the SSP appeared to experience a greater intensity in their conflicts of relevance (H5).

In both programs the goals of research did not appear subject to any major changes in relevance, as demonstrated by the general stability of all the scientists' structures of relevance. In the SSP some changes in relevance did occur, as indicated by the displacement of some of the higher level goals from a context of research to a context of legitimation. In the DOP however, although the more general goals of the program were partially established in the context of legitimation, they did not become as highly separated from the context of research as did the higher level goals of the SSP (H9).

In Chapter 8 a number of hypotheses that were specifically oriented towards a comparison of the two programs were explored on the basis of quantitative data abstracted from the case studies:

Scientific research was shown to vary in its orientation towards social application (H13).

Practice oriented research (for example, much of the research in the SSP) was found to be more highly constrained by social, economic and political factors than basic research - for example, the DOP (H14).

The levels of institutionalisation of the contexts of legitimation of the research programs was found to be positively correlated with the levels of scientific marginality of the programs (H15).

The levels of institutionalisation of the contexts of legitimation of the research programs was found to be positively correlated with the levels of orientation of program members towards the more technical goals of the programs (H16).

The levels of institutionalisation of the contexts of legitimation of the research programs was found to be positively correlated with the levels of practice orientation of research in the programs (H18).

The material discussed in Chapter 8 provided support for most of the hypotheses that were postulated in Section 4.9-1. Two of the hypotheses were however, not fully supported and one hypothesis was completely unsupported. The material demonstrated that regarding H6, there was insufficient data to fully support the (sub-)hypothesis that the levels of the cognitive field of the researchers were interpenetrating; regarding H10, change in the goals of scientists was of a restricted nature; and regarding H17, the data did not support a postulated positive correlation between the level of institutionalisation of the context of legitimation of the research programs and the levels of orientation of scientists towards higher level goals. The material did support a revised version of the first two hypotheses, viz,

Scientific research occurs in the context of a structured cognitive field which consists of different levels of structure: metaphysical, theoretical, subject concern, and technical levels (H6*); and,

The research goals of scientists change over time, but in the lifetime of a particular research program this change occurs more as a process of sedimentation of new goals into established structures than by the replacement of old goals (H10*).

9.3 General conclusions

The scope of this thesis has been very large since I have attempted to provide a general framework of analysis with the potential of being extended beyond the focus on the physical sciences contained in this thesis to the analysis of all sciences. This theoretical framework is not however, complete - the goal of a "complete" or "finalised" theory is, of course, an ideal which has, fortunately for the sake of personal and cultural growth, never been achieved in the social sciences. Since part of the empirical work required the demonstration of a descriptive framework which ranged in scope from macro-structures to micro-structures, the case studies were necessarily extensive. But, given the limitations of an individually conducted research program, the empirical work has only explored particular aspects of the theory. In addition, both of the research programs studied were relatively marginal to the scientific mainstream and were not a fully representative sample of research in the physical sciences (both programs are still nonetheless, considered to be sufficiently typical to enable generalisations on their bases). The first general conclusion is then that more work needs to be done; some suggestions along these lines will be made in a separate section.

This thesis incorporates three levels of analysis - in Mannheim's terms, an expressive level, an objective level, and an historical evidential level of meaning. That is, in concrete terms, the thesis is a synthesis of individual testimony, observations which were independent of particular individual's universes of meaning, and theoretical structures and interpretations which are located in historical time and which reflect something of "the spirit of the age".

Although the thesis does contain much historically conscious material, the work has not been primarily historical in orientation - consequently, some of the thesis - particularly the natural science dimension of the case studies - is in parts a-historical. This has been largely a consequence of the choice of a frame of reference which tended to focus attention on the relatively immediate production of research findings - such a framework, whilst by no means excluding the detailed history of aspects of the sciences which provide structures of relevance for particular research programs, does need to be expanded in the historical dimension. Such a movement was partially suspended because of the enormous volume of information about contemporary research events which required prior analysis.

Nonetheless, this thesis demonstrates that it is possible for an individual investigator to move between in depth analysis in a specialised sub-universe (such as regions of the natural sciences) and theoretical synthesis in another specialised domain (sociology). Whereas this movement is, in principle, precisely what any empirical sociology of a specialised sub-universe should involve, the extent to which in depth analyses by sociologists of the form and content, structure, meaning and process within fields of the natural sciences has occurred has tended to be highly constrained by the ability of sociologists to actually "penetrate" the day-to-day reality of the natural scientific lifeworld. The extent to which I have been able to penetrate regions that are, even to the sociologist with a scientific background, difficult to fathom is reflected in the generally high level of interaction I have been able to achieve with my respondents over much of the material that has been discussed in this thesis.

The success of the thesis in demystifying the "black box" of the laboratory is to a considerable extent, due to the methodology that has been employed. The "method of repeated feedback" that was developed for the fieldwork is efficient resource-wise and accurate given the in-built test of validity that it has. Nonetheless, further refinements in the method are possible - for example, the analysis has not been scrutinised by experts outside the particular chosen research programs (although such outsiders were canvassed in the process of the research). The generation of a deeper understanding of the history of the particular sciences being researched would seem to require a higher level of participation by scientists "outside" individual research programs.

The analysis has also been restricted in the extent to which the metaphysical and ideological levels of scientific research have been explored. The identification of articulated belief systems in contexts of legitimation of research is a first step in that direction, but these types of structures mostly reflect scientists' consciously articulated beliefs and may not begin to deal with the subtler forces which shape scientific world views. Some of these forces are most likely effective at the level of the unconscious and may, for that reason, be somewhat inaccessible. Nonetheless, any thorough going sociology of knowledge of the natural sciences will need to develop techniques of research which can be effective at that level, too.

As a sociology of knowledge this thesis is lacking of an analysis of the way in which research findings are transformed into accredited scientific knowledge. The general processes involving the judicious selection of material for publication which proceed via the constraints of editorial policies, professional opinion, public opinion, etc., are well known as part of the taken-for-granted experience of anyone trying to publish in a specialised field; nonetheless, greater understanding of the processes by which "knowledge" is accredited would be a valuable addition to a study such as the present one which tends to focus on the immediate production of research findings.

In this thesis the subject of the relevance of scientific research has been explored through the general categories of Aflred Schutz, including the concepts of motivational, thematic and interpretational relevancy. Particular emphasis has been placed on the in-order-to motive of research through an analysis of the goals of research. This focus was consistent with a theory that placed great stress on the nature of research as "projects of action", but a somewhat partial account of the general relevancy of research has been a consequence. Aspects of the thematic and interpretational relevancy of scientists' research were treated as a matter of necessity in generating adequate research accounts, but the "because" aspect of motivational relevancy was neglected. This neglect was partially a consequence of the sociological frame of reference that has been adopted - which in the nature of disciplinary constraints tends to make "psychological" questions irrelevant, and partially as a consequence of a lack of sufficient time and other resources necessary to fully explore individual psyches. A more comprehensive understanding of scientific research will need to integrate all aspects of the relevancy of research, despite traditional disciplinary norms.

9.4 Suggestions for future work

It has not been possible to pursue all the potentially fruitful questions that have arisen during the production of this thesis. Some of these questions were largely suppressed in the interests of "relative closure" and some of these questions have emerged largely on the basis of the knowledge gained from the process of ordering a large amount of information - the difference between the two processes often being hard to determine. These "loose ends" that remain fall roughly into two categories: those concerned with theoretical and empirical questions that relate to sociology in general, and those that relate more specifically to the sociology of science. These "questions" have been listed below as suggestions for future work along lines that extend from this thesis.

(a) Suggestions related more specifically to the sociology of science

1. Similarly based in depth analyses of research programs in different disciplines of the natural sciences to enable a more detailed comparatively based assessment of the various hypotheses generated in this thesis.

2. An extension of the method of repeated feedback to enable the production of information of a greater historical dimension. More specifically, what is required is a method of generating oral histories of specialised areas of research which may not be well documented.

3. A more detailed examination of the relationship between different levels of the cognitive field of research - that is, a more detailed exploration of the relationship between metaphysical, theoretical, subject concern and technical levels of research.

4. A more detailed investigation of the nature of serendipity in research and the relationship between its level of occurrence and the level of institutionalisation of research.

5. Investigation of the processes of production of accredited scientific knowledge. The relationship between the production of research findings which has tended to be the major focus of this thesis needs to be linked more precisely to processes which shape the socially accredited stocks of knowledge which come to be shared by scientists.

6. An investigation of the relationship between the level of institutionalisation of the context of legitimation of research programs and the content of stocks of knowledge shared by non-scientific reference groups (such as pressure groups and the "general public").

(b) <u>Suggestions of a more general sociological interest</u>

7. An extension of the theory and method which have been developed in this thesis into disciplines other than the natural sciences - for example, the social sciences and more metaphysically concerned areas of research. An investigation of fields where questions of value and meaning are more central to the actual processes of research is necessary if only to see whether the way in which research has been postulated in this thesis as a type of social action is relevant outside of fields of enquiry which tend to remain at levels of analysis which avoid confronting questions dealing with the reality and importance of human values and consciousness.

8. Further theoretical research on the alleged non-dialecticity of phenomenological theory - see for example, Smart [1976] and Gorman [1975]. In the context of the theoretical structure of this

thesis the subject of relevance requires more attention, particularly with respect to the relationship between Schutz's concepts of "in-order-to" and "because" motives which may be, as Bernstein [1979:232] has pointed out, artificial and non-dialectic. None of the above-mentioned critics has, it should be pointed out, developed a particularly well substantiated theoretical critique of Schutz's work.

9. A more detailed investigation of the social-psychological mechanisms of alternation between different provinces of meaning, including contexts of research and contexts of legitimation.

INDEX TO APPENDICES

APPENDIX 1: Relationship between levels of the autonomy index and individual autonomy

A = F - R, where

A = Numerical level of autonomy index;
F = Numerical level of influence of an individual over the formation of a particular goal;
R = Numerical level of influence of the goal over individual research

Dependent Variables		Independent Variable	Interpretation of individual's position
F	R	A	
H	L,M	+)	Exerting influence over others in group
M	L	+)	
H	H	0)	Relative individual autonomy
L	L	0)	
M	M	0)	
L	M,H	-)	Being influenced by others in group or outside of group
M	H	-)	

Key to levels: H = High level)
M = Medium level) all levels > 0
L = Low level)

APPENDIX 2: List of individuals interviewed prior to selection of individual research programs for in depth study

The scientists and engineers listed below were selected from a network of contacts that had been established during the periods of my undergraduate training in chemical and fuel engineering (1970-1973) and my subsequent employment as a Professional Officer in a university physical chemistry department (1974). Informal and open ended interviews were held with the following people in February, 1977:

Associate-Professor R. Aroney, Department of Organic Chemistry, Sydney University

Professor R. Bilger, Department of Mechanical Engineering, Sydney University

Associate-Professor N.A. Gibson, Department of Inorganic Chemistry, Sydney University

Associate-Professor Hunter, Department of Physical Chemistry, Sydney University

Mr. A.M. Johnston, School of Physics, Sydney University

Dr. G. Sergeant, Department of Fuel Engineering, University of New South Wales

Dr. I. Smith, Department of Mineral Chemistry, C.S.I.R.O.

Ms. G. Sudlow, Department of Clinical Pharmacology, University of New South Wales

Professor D. Wade, Department of Clinical Pharmacology, University of New South Wales

Dr. B. Window, School of Physics, University of Sydney.

APPENDIX 3: First round summary and questionnaire administered to members of the SSP

Letter:

"The University of Wollongong
Department of Sociology.

Ref: TJ/MM

7th March, 1978.

A CONTINUATION OF MY SOCIOLOGICAL RESEARCH INTO YOUR RESEARCH PROGRAM:

Attached to this note there are a series of Figures which represent my attempt to reconstruct events on your selective surfaces related research (up to about April, 1977). The analysis I've presented here is largely a product of our "first round" discussions, but I may have made any number of serious mistakes, distortions, or ommissions - naturally enough I take full responsibility for these errors.

I'm particularly interested in these errors and at your convenience I would appreciate some feedback about them. If possible I'd like to meet with you fairly soon to discuss your impressions and criticisms of my analysis. Hopefully this will enable me to get things straight (where necessary) and resubmit to you a revised version which may meet with substantial approval from all those involved in the research program.

Following this I'd like to meet with you on two further occasions - the first time to ask you some fairly specific questions about the reconstruction of events, and the second time in a joint meeting with all the members of the program (if this can be arranged). This strategy of meetings represents my attempt to gain both your individual interpretations and an "objective" account that is based on some measure of consensus."

APPENDIX 3 (cont.)

FIGURE 1 is intended to be a general description of the "theoretical landscape" of the selective surfaces program as perceived by the program members. The term "theoretical landscape" is meant to cover the scientific law, models, examples of theoretical applications and "facts" that are directly referred to and which provide a working background for the research conducted on the program. In other words, this "landscape" is a structure of knowledge which is instrumental in providing a level of direction, content and meaning for your research. It is assumed that at the level of generality indicated the "landscape" will be similarly perceived by all program members. If this is not the case, please indicate where the differences lie.

This notion of theoretical landscape is obviously broad in scope, but I am attempting to distinguish, as far as possible, between those theoretical aspects that you are conscious of using and referring to and a whole structure of knowledge which could extend through many disciplines and many years of education. A further distinction is being drawn here between "theory" and the more immediate "practical" concerns of your program. I have attempted to express these latter concerns in the goals listed in Figure 2.

Any corrections, additions, or general comments?

APPENDIX 3 (cont.)

FIGURE 1: THEORETICAL LANDSCAPE OF THE SELECTIVE SURFACES PROGRAM (as of April, 1977).

Level of Theoretical Landscape	Theoretical Components and Their Professional Orientation*			
		Scientific		Engineering
Discipline	T1	Physics		
	T2			Mechanical Engineering
Sub-Discipline	T3	Solid State Physics		
	T4		Materials Science	
Specialty	T5	Theoretical Modelling of surfaces		
	T6	Selective surfaces		
	T7			Heat transfer
	T8		Solar energy utilisation	
Program		Properties of:		
	T9	Metal mesh absorbers		
	T10	Globular metal films		
	T11	Cermet surfaces		
	T12	Graded interference layers		

* The components have been listed under their most effective professions. Where the components are oriented to both the professions they have been listed in the centre of the Table.

APPENDIX 3 (cont.)

FIGURE 2 is a list of research goals. It has been attempted to order this list in an approximate order of increasing "closeness" to day-to-day research. As before with the "landscape" in Figure 1, it is assumed that these goals are mutually perceived. If this is not the case, please indicate where any differences lie.

Any corrections, additions, or general comments?

APPENDIX 3 (cont.)

FIGURE 2: A LIST OF THEORETICAL AND TECHNICAL GOALS THAT EFFECTED THE DIRECTION OF THE SELECTIVE SURFACES PROGRAM (up to April, 1977)

Level of Theoretical and Technical Goals	Professional Orientation*			
		Scientific		Engineering
Discipline				
Sub-Discipline	G1	Establish a solid state physics research front		
Specialty	G2		Establish a solar energy research front	
	G3		Establish a solid state oriented solar energy research front	
Program	G4	Incorporate selective surface research with solar energy research		
	G5	Develop a new, efficient selective surface		
	5.1		Develop a refractory selective surface	
	G6		Develop a commercially viable collector which employs the new selective surface	
	6.1		Develop an alternative to the established flat plate collector	
	6.2		Develop a model of the collector system	
	6.3			Develop a system which produces heat in the intermediate rage of 100-300°C
	G7	Investigate the properties of grids as selective surfaces		
	G8	Investigate the properties of globular metal films		
	G9	Investigate the properties of cermet surfaces		
	G10	Investigate the properties of graded interference layers		
	G11		Develop ways of applying selective surfaces to substrates	
	11.1		Develop the sputtering technique	

* The components have been listed under their most effective professions. Where the components are oriented to both the professions they have been listed in the centre of the Table.

APPENDIX 3 (cont.)

FIGURE 2 (cont.)

Level of Theoretical and Technical Goals	Professional Orientation		
	Scientific		Engineering
	11.2		Develop a long sputtering chamber
	G12		Develop an evacuated, tubular sputtered glass collector
	G13		Develop commercial applications for a working solar array
	13.1		Work out a heat extraction system
	13.2		Work out a configuration of collectors and concentrating mirrors
	13.3	Check degradation mechanisms of the materials involved	
	13.4		Develop an air conditioning system using the solar output

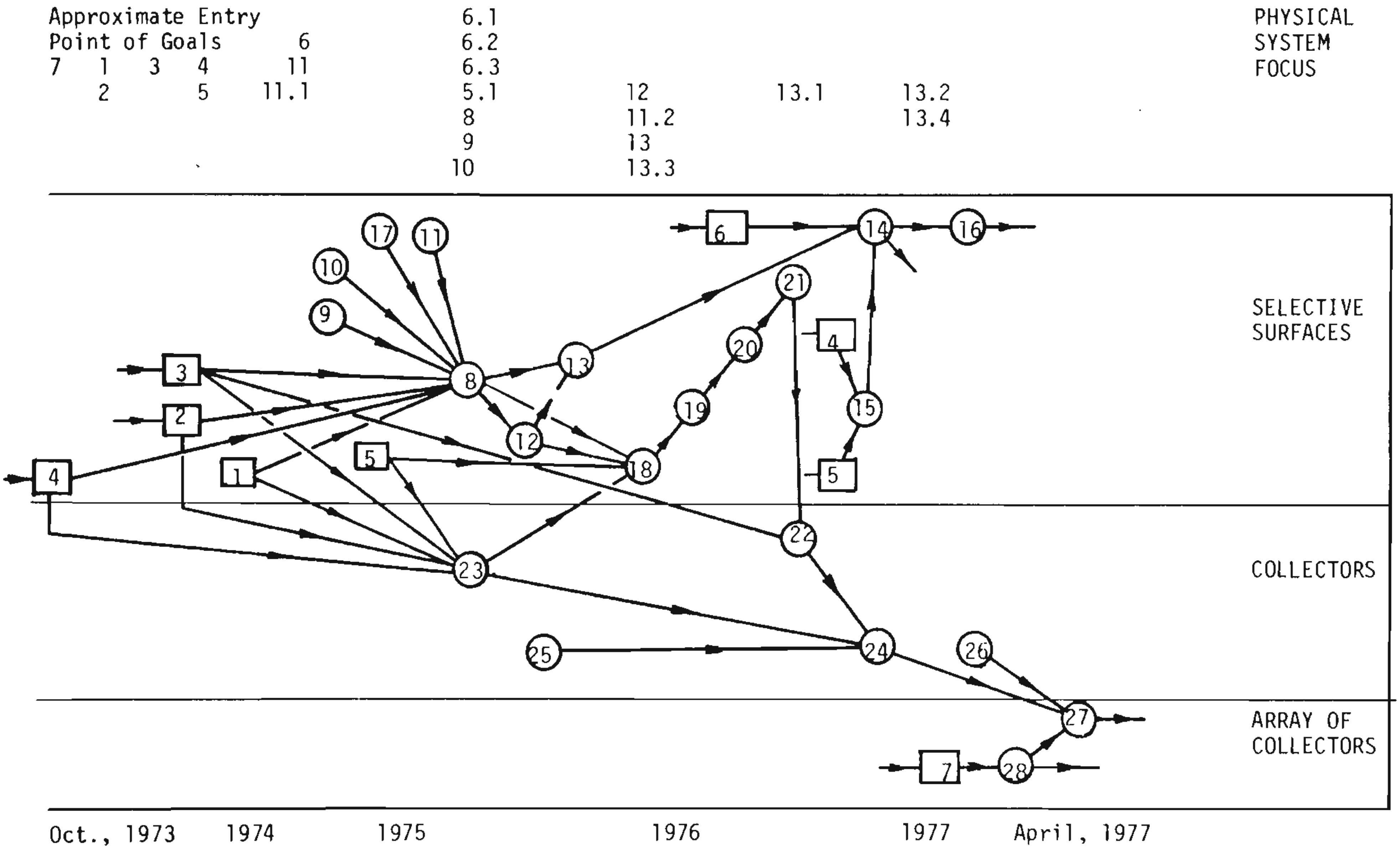
APPENDIX 3 (cont.)

FIGURE 3 is a flow chart of significant research events in the evolution of the selective surfaces program. The events being considered here are in the class of important observations, hypotheses, ideas, strategies, theoretical developments, technical developments, etc. The key to the numbered events is on the page after the diagram. The goals referred to on the top of the diagram are numbered the same as in Figure 2.

Any corrections, additions, or general comments?

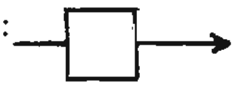
APPENDIX 3 (cont.)

FIGURE 3: FLOW CHART OF SIGNIFICANT RESEARCH EVENTS IN THE EVOLUTION OF THE SELECTIVE SURFACES PROGRAM



APPENDIX 3 (cont.)

KEY TO FIGURE 3: Significant research events in the evolution of the selective surfaces program. (Note: this list is not in chronological order).

LINES OF RESEARCH: 

1. Superconductivity:--------
2. Neutron diffraction:--------
3. Magnetism:--------
4. Metal mesh absorbers (Ph.D):--------
5. Theoretical physics:--------
6. Graded interference layers:--------
7. Refrigeration (Ph.D):--------

PARTICULAR EVENTS: 

8. Metal blacks established as promising selective surfaces.
9. Metal meshes rejected as commercially viable selective surfaces on account of optical performance problems.
10. Emiss ometer developed.
11. Absorptometer developed.
12. Gold black rejected as surface candidates because of its instability at high temperatures.
13. Cermets established as surface candidates.
14. Theoretical advances in cermet theory: the traditional approach in predicting optical constants from the Maxwell-Garnett formula is found to be in error in some cases. An alternative "exact" method using a technique invented by Lord Rayleigh is developed.
15. Dispute with Chris Ceders (US) about his theoretical treatment of cermet dielectric constants.
16. Theoretical understanding of eraser particles developed.
17. ------------ (NML) brings to attention the serendipitous discovery in 1952 of the selective nature of gold blacks.
18. Reactive sputtering technique developed for a range of materials.
19. Oxides rejected as selective surfaces because of their instability at high temperatures.
20. Metal carbides established as likely prospects (with the caveat that there are no "magic" carbides.
21. A sputtered iron carbide surface is successfully deposited.
22. Sputtering chamber constructed.
23. Flat plate system rejected.
24. An evacuated, tubular glass collector with a sputtered iron carbide selective surface is constructed.

APPENDIX 3 (cont.)

KEY TO FIGURE 3 (cont.)

25. Owens Illinois (US) evacuated glass collectors assessed as basis for selective surface collectors.
26. A heat exchanger for the collector is developed.
27. An array of collectors is assembled and demonstrated to Neville Wran and the press.
28. Air conditioning device proposed.

APPENDIX 3 (cont.)

FIGURE 4 is an attempt to understand the goals of research in terms of social factors. "Social factors" is meant in a broad sense to include political and economic factors, as well as factors arising from human interaction. This kind of analysis is, of course, not the full story since the goals express various theoretical interests and other goals, and really, are only fully comprehensible in terms of these other factors. At this stage, however, I'd like to focus on the kinds of social factors present in the Figure.

Any corrections, additions, or comments?

APPENDIX 3 (cont.)

FIGURE 4: PRELIMINARY ANALYSIS OF THE GOALS OF THE SELECTIVE SURFACES PROGRAM

Goals* - in approx. order of appearance	Summary explanation of significant SOCIAL factors contributing to the establishment and evolution of goals
G7	1. This was the major goal of -------'s Ph.D research.
G1	1. Solid state research was perceived by -------- to be something of a gap in the School's research capacity.
G2	1. Solar energy research was perceived by -------- to be something of a gap in the School's research capacity.
	2. Solar energy research general considered to be (in the Physics School and in the University generally) capable of making a worthwhile social and economic contribution to society.
	3. Solar energy research can be sufficiently commercially viable to promise funding and possible, direct economic returns (e.g., patents, consultancies and commercial sales).
	4. Such research being potentially of great public and political concern, could further legitimate Physics as a discipline and also legitimate the S.U. Physics School.
	5. The research could also function as a common interest and bond between the Physics School and the Mechanical Engineering, Chemical Engineering and Biochemistry Departments in the University (particularly through the mooted Energy Centre).

* See Figure 2 for the key to the numbering of the goals.

APPENDIX 3 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant SOCIAL factors contributing to the establishment and evolution of goals
G3	1. A good way of "killing two birds with one stone": The combination of two goals in the one program is mutually reinforcing in that the solid state research angle, particularly legitimates the program from the point of view of the physics community, whereas the solar energy angle particularly legitimates the program from the point of view of more public-spirited "non-physicists", who might have interest in the program.
	2. This kind of program goal is consistent with --------'s entrepreneurial style.
	3. Given that this kind of research has "popular" interest the goal might also help deal with the student recruitment problems that the Physics Department is experiencing.
G4	1. A condition of employment of -------- and --------. Although selective surface research is compatible with the previous research interests of these program members, it nonetheless represents (to varying extents) a re-direction of their earlier research work.
	2. From the point of view of the program members this is a fruitful line of research because it is publishable physics and may lead to the development of their "own" new surface.
G5	1. Having one's "own" surface is important from the point of view of protection from competition and enhanced publication prospects, as well from the point of view of curiosity over the unknown.

APPENDIX 3 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant SOCIAL factors contributing to the establishment and evolution of goals
	2. This "ownership" is important commercially, in that patenting and external funding "attractiveness" depend, to a large extent, on this. This is then an economic legitimator for the program.
G6	1. This goal provides economic prospects for the university (which has been having financial problems) and also, consequently, for the Physics Department. This is the major economic legitimator for the program - economic legitimation being particularly relevant in an environment where research must increasingly be shown to be worthwhile. One desirable outcome might be that this goal might attract external funding.
G11	Logical development from G6.
G11.1	1. The sputtering technique is a particular interest of -------- following earlier research of his in England where the technique was developed as part of his work on superconductivity.
G6.1	1. Flat plate collector research is a well worked and not particularly exciting area of research so this goal is desirable from the point of view of avoiding direct competition with established programs of research, by establishing a line of research with a different approach.
G6.2	1. A mathematical model of the collector is important if optimisation studies, a part of the process of "development", are ever to be undertaken.

APPENDIX 3 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant SOCIAL factors contributing to the establishment and evolution of goals
G6.3	1. For a solar collecting system to be more commercially viable than existing systems which usually just warm water for domestic use, a "low grade" temperature output of 100-300°C seemed promising, and possible. Such a thermal output increases the range of potential uses and could even perhaps provide low grade steam for eventual electricity generation.
G5.1	Logical development from G6.3.
G8, 9, 10	1. This line of research could result in improved selective surfaces.
G12	Largely an extension of the previous goals.
G11.2	Logical extension of G11.1 and G12.
G13	An extension of the previous goals.
G13.3	Logical extension of G13.
G13.1	Logical extension of G6.3.
G13.2	Logical extension of G13.
G13.4	1. An air conditioning unit operating from the thermal output of the program's collector has a number of economic and social advantages over the conventional domestic water heating applications: i. There is a large domestic demand for air conditioners; ii. Solar air conditioning units tend to avoid competing with the electricity commission in the same way that solar water heaters do

APPENDIX 3 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant SOCIAL factors contributing to the establishment and evolution of goals
	ii (cont.) (by virtue of enhancing rather than detracting from electricity demand curve characteristics - in particular solar air conditioners could contribute to lessening mid-day demands).

APPENDIX 4: Second round summary and questionnaires administered to members of the SSP

Letter:

"The University of Wollongong.

14th September, 1978.

Dear

Here are some of the results of my research to date. These results represent my attempts to establish a consensus about the various aspects of the selective surfaces program over the period early 1974 to April 1977 (approximately). The results are particularly based on our last discussion where we talked about my first attempt to present a picture of your joint research over the abovementioned period of time.

I have enclosed a questionnaire based on these results and I will be most interested in your response.

Kind regards and thanks for your co-operation.

Yours sincerely,

Tom Jagtenberg (sgd.)

Department of Sociology,
UNIVERSITY OF WOLLONGONG."

Encls.

APPENDIX 4 (cont.)

FIGURE 1: Theoretical "landscape" of the selective surfaces program (As of April, 1977; round two synthesis).

Level of Theoretical Landscape	Theoretical Components and Their Professional Orientation*		
	Scientific		Engineering
Discipline	T1 Physics		
	T2		Mechanical Engineering
Sub-Discipline	T3 Solid State Physics		
	T4	Materials Science	
	T5	Thin Film physics	
	T6	Vacuum technology	
	T7	Heat transfer	
Specialty	T8	Selective surfaces	
	T9	Solar energy utilisation	
Program	Properties of:		
	T10 Geometric selective absorbers:		
	10.1 metal mesh absorbers		
	10.2 globular metal films		
	T11 Cermet surfaces		
	T12 Graded interference layers		

* The components have been listed under their most effective professions. Where the components are oriented to both the professions they have been listed in the centre of the Table.

APPENDIX 4 (cont.)

FIGURE 2: A list of theoretical and technical goals that effected the direction of the selective surfaces program. (Up to April, 1977; round two synthesis).

Level of Theoretical and Technical Goals	Professional Orientation*				Approximate Date of Emergence of Goal
		Scientific		Engineering	
Discipline Sub-Discipline	G1	Establish a solid state physics research front			Nov., 1973
Specialty	G2		Establish a solar energy research front		Nov., 1973
	G3		Establish a solid state oriented solar energy research front		Dec., 1973
Program	G4		Develop an alternative to the established flat plate collector		
	G5	Incorporate selective surface research with solar energy research			Jan., 1974
	G6	Develop a new, efficient selective surface			Jan., 1974
	6.1		Develop a refractory selective surface		May, 1975
	G7			Develop a system which produces heat in the intermediate range of 100-300°C	
	G8		Develop a commercially viable collector which employs the new selective surface		Nov., 1974

* The components have been listed under their most effective professions. Where the components are oriented to both the professions they have been listed in the centre of the Table.

APPENDIX 4 (cont.)

FIGURE 2 (cont.)

Level of Theoretical and Technical Goals	Professional Orientation			Approximate Date of Emergence of Goal
	Scientific		Engineering	
Specialty (cont.)	G9		Develop a model of the collector system	May, 1975
	G10	Investigate the properties of geometric selective absorbers:		
		10.1 metal mesh absorbers (i.e. grids)		Oct., 1973
		10.2 globular metal films		May, 1975
	G11	Investigate the properties of cermet surfaces		May, 1975
	G12	Investigate the properties of graded interference layers		May, 1975
	G13		Develop ways of applying selective surfaces to substrates	Nov., 1974
	13.1		Develop the sputtering technique	Nov., 1974
	13.2		Develop a long sputtering chamber	Nov., 1975
	G14		Develop an evacuated, tubular sputtered glass collector	Nov., 1975
	G15		Develop commercial applications for a working solar array	Nov., 1975
	15.1		Work out a heat extraction system	April, 1976
	15.2		Work out a configuration of collectors and concentrating mirrors	Dec., 1976

APPENDIX 4 (cont.)

FIGURE 2 (cont.)

Level of Theoretical and Technical Goals	Professional Orientation		Approximate Date of Emergence of Goal
	Scientific	Engineering	
Specialty (cont.)	15.3 Check degradation mechanisms of the materials involved		Nov., 1975
	15.4	Develop an air conditioning system using the solar output	Dec., 1976

APPENDIX 4 (cont.)

FIGURE 4: Preliminary analysis of the goals of the Selective Surfaces Program.

Goals* - in approx. order of appearance	Summary explanation of significant SOCIAL factors contributing to the establishment and evolution of goals
G10a.	1. This was the major goal of --------'s Ph.D research.
G1	1. Solid state research was perceived by -------- to be something of a gap in the School's research capacity.
	1. Solar energy research was perceived by -------- to be something of a gap in the School's research capacity.
	2. Solar energy research general considered to be (in the Physics School and in the university generally) capable of making a worthwhile social and economic contributions to society.
	3. Solar energy research can be sufficiently commercially viable to promise funding and possible, direct economic returns (e.g., patents, consultancies and commercial sales).
	4. Such research, being potentially of great public and political concern, could further legitimate Physics as a discipline and also legitimate the Physics School.
	5. The research could also function as a common interest and bond between the Physics School and the Mechanical Engineering, Chemical Engineering and Biochemistry Departments in the University (particularly through the mooted Energy Centre).

* See Figure 2 for the key to the numbering of the goals.

APPENDIX 4 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant . . .
G3	1. A good way of "killing two birds with one stone": The combination of two goals in the one program is mutually reinforcing in that the solid state research angle, particularly legitimates the program from the point of view of the physics community, whereas the solar energy angle particularly legitimates the program from the point of view of more public-spirited "non-physicists", who might have interest in the program.
	2. This kind of program goal is consistent with ------'s intrepreneurial style.
	3. Given that this kind of research has "popular" interest the goal might also help deal with the student recruitment problems that the Physics Department is experiencing.
G5	1. A condition of employment of -------- and --------. Although Selective Surface research is compatible with the previous research interests of these program members, it nonetheless represents (to varying extents) a redirection of their earlier research work.
	2. From the point of view of the program members this is a fruitful line of research because it is publishable physics and may lead to the development of their "own" new surface.
G6	1. Having one's "own" surface is important from the point of view of protection from competition and enhanced publication prospects, as well from the point of view of curiosity over the unknown.

APPENDIX 4 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant . . .
G6 (cont.)	2. This "ownership" is important commercially, in that patenting and external funding "attractiveness" depend, to a large extent, on this. This is then an economic legitimator for the program.
G8	1. This goal provides economic prospects for the university (which has been having financial problems) and also, consequently, for the Physics Department. This is the major economic legitimator for the program - economic legitimation being particularly relevant in an environment where research must increasingly be shown to be worthwhile. One desirable outcome might be that this goal might attract external funding.
G13	Logical development from G8.
13.1	1. The sputtering technique is a particular interest of --------, following earlier research of his in England where the technique was used as part of his work on superconductivity.
G14	1. Flat plate collector research is a well worked and not particular exciting area of research so this goal is desirable from the point of view of avoiding direct competition with established programs of research, by establishing a line of research with a different approach.
G9	1. A mathematical model of the collector is important if optimisation studies, a part of the process of "development", are ever to be undertaken.

APPENDIX 4 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant . . .
G7	1. For a solar collecting system to be more commercially viable than existing systems which usually just warm water for domestic use, a "low grade" temperature output of 100-300°C seemed promising, and possible. Such a thermal output increases the range of potential uses and could even perhaps provide low grade steam for eventual electricity generation.
G6	Logical development from G7.
G10b, 11, 12	1. This line of research could result in improved selective surfaces.
G14	Largely an extension of the previous goals.
G13.2	Logical extension of 13.1 and G14.
G15	An extension of the previous goals.
G13.3, 15.3	Logical extension of G15.
G13.1, 15.1	Logical extension of G7.
G13.2, 15.2	Logical extension of G15.
G13.4, 15.4	1. An air conditioning unit operating from the thermal output of the program's collector has a number of economic and social advantages over the conventional domestic water heating applications: i. There is a large domestic demand for air conditioners; ii. Solar air conditioning units tend to avoid competing with the electricity commission in the same way that solar water heaters do (by virtue of enhancing rather than detracting from electricity demand curve characteristics -

APPENDIX 4 (cont.)

FIGURE 4 (cont.)

Goals - in approx. order of appearance	Summary explanation of significant . . .
	ii. (cont.) in particular solar air conditioners could contribute to lessening mid-day demands). 2. This is considered (by --------, the leader of the solar energy group and ---- the head of the Mechanical Engineering Department) to be a line of research appropriate for a Ph.D project which could be conducted from the disciplinary basis of mechanical engineering, but in conjunction with the Solar Energy Group.

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS.

1. On the basis of my last round of discussions there appears to be a reasonable level of consensus about the theoretical "landscape" of the selective surfaces program as perceived by program members.

   Do you have any disagreements with the picture presented in Figure 1? Place your comments on the Figure, if that's more convenient.

2. Also on the basis of our last round of discussions, there appears to be a high level of consensus about the major research goals that were effective on the program over the period October, 1973 to April, 1977 (approximately).

   However, I do not have a clear impression of the relative importance of these goals. Towards this end I have presented in Figure 2 a list of research goals that appears to reflect a consensus amongst --------, --------, --------, and -------- (that is, amongst all those whom I have had discussions with about the goals). The list is substantially the same as the list you have already seen and commented upon; the small amendments are a product of the new information I gathered in the last round of interviewing.

   Q.2.1: Do you still agree with the formulation of the goals as presented in Figure 2? That is, were these goals a reality as far as you were concerned, and are they a relatively complete and accurate expression of the research ends towards which you were moving during the period under consideration?

   Place your comments on the Figure if that's more convenient.

   Q.2.2: Is it possible for you to list the goals in an order of priority as far as you were concerned, and as far as the group was concerned in the period under study? Rank the goals numerically: 1, 2, 3 . . . n.

   If there were any major changes in priorities over the period under study, please indicate where.

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS (cont.)

Goals	Personal Priority	Priority for the Group (if different)
G1		
G2		
G3		
G4		
G5		
G6		
G7		
G8		
G9		
G10		
G10.1		
G10.2		
G11		
G12		
G13		
G13.1		
G13.2		
G14		
G15		
G15.1		
G15.2		
G15.3		
G15.4		

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS (cont.)

Q.2.3: Which were the goals that most affected your work? Here I'm interested in the relative extents to which you were involved in working towards some goals rather than others. Scale your involvement on a 0-5 basis where the numbers have the following meanings:

0 or 1) low

2 or 3) medium

4 or 5) high involvement

If there were any changes over the period, please indicate where.

Goal	Level of Involvement (0-5)
G1	
G2	
G3	
G4	
G5	
G6	
G7	
G8	
G9	
G10	
G10.1	
G10.2	
G11	
G12	
G13	
G13.1	
G13.2	
G14	
G15	
G15.1	
G15.2	
G15.3	
G15.4	

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS (cont.)

Q.2.4: How many days/week on average would you say you were engaged on selective surfaces related research in the period under consideration?

Q.2.5: How would you classify the type of your involvement (on average):

i. managerial and/or experimental?
ii. creative and/or routinely technical/
iii. any other way?

Q.2.6: Have there been any research goals that have emerged as important to the direction of the selective surfaces related research since October 1976? Have any of the priorities changed because of these new goals?

Goal	Personal Priority Now (September, 1978)	Priority for Group Now
G1		
G2		
G3		
G4		
G5		
G6		
G7		
G8		
G9		
G10		
G10.1		
G10.2		
G11		
G12		
G13		
G13.1		
G13.2		
G14		
G15		

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS (cont.)

Q.2.6:

Goal	Personal Priority Now (September, 1978)	Priority for Group Now
G15.1		
G15.2		
G15.3		
G15.4		
+ new goals (give approx. date of emergence)		
+		
+		

Q.2.7: (i) Have any of the goals been achieved?

(ii) Using the table on the next page, how would you rate progress towards the various goals - at the beginning of the program (early, 1974), at April, 1977, and now (September, 1978)?

If possible, use a 0-5 scale where the numbers have the following meanings:

0 or 1 — low

2 or 3 — medium

4 or 5 — high progress

Note: A rating of 5 is taken to mean that the goal has been successfully achieved.

(iii) Have there been any publications or other products that have been the result of work towards any of the goals in particular, or perhaps towards a group of goals? I have enclosed a list of publications

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS (cont.)

Q.2.7: (iii) cont.
that might be relevant (pp.viii-xii). Please list the appropriate number of the publication beside the appropriate goal/s.

Goal	Early '74	April, '77	Sept. '78	Publications or other products?
G1				
G2				
G3				
G4				
G5				
G6				
G7				
G8				
G9				
G10				
G10.1				
G10.2				
G11				
G12				
G13				
G13.1				
G13.2				
G14				
G15				
G15.1				
G15.2				
G15.3				
G15.4				

Q.2.8: Which people were involved in the formulation of each of these goals? Where possible rank the influence of the people 0-5 where the numbers have the following meanings:

0 or 1 — low

2 or 3 — medium

4 or 5 — high influence

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS (cont.)

Q.2.8 (cont.)

INFLUENCE OF PEOPLE IN THE FORMATION OF GOALS

Goals
G1
G2
G3
G4
G5
G6
G7
G8
G9
G10
G10.1
G10.2
G11
G12
G13
G13.1
G13.2
G14
G15
G15.1
G15.2
G15.3
G15.4

Q.2.9: Would you say that there are any goals (research or other) that have particular significance in characterising:

i. the overall direction, and
ii. the general "feeling" (as you felt it) of the research over the period under consideration?

Are there any differences now?

Q.2.10: Do you have (or did you have) any other goals (these could be political, economic, social or psychological in nature) that could be of importance to my understanding of the research in the period under consideration?

APPENDIX 4 (cont.)

RESULTS AND QUESTIONS (cont.)

Q.2.11: How much sense do you think there is in dividing off certain research activities from others (as I have done in looking at the selective surfaces program)? What other research were you conducting at the same time, and if you were involved with other research, how interconnected were your different projects?

****

APPENDIX 4 (cont.)

Letter:

"The University of Wollongong.

Ref: TJ/MM

14th September, 1978.

Dear

As promised here's a revised version of the "flow diagram" that you saw and commented upon earlier this year. I've attempted to incorporate as many of the suggestions I received as possible, but if this version still does not adequately capture events as you remember them, please add further corrections where necessary.

Kind regards, and thanks for your co-operation,

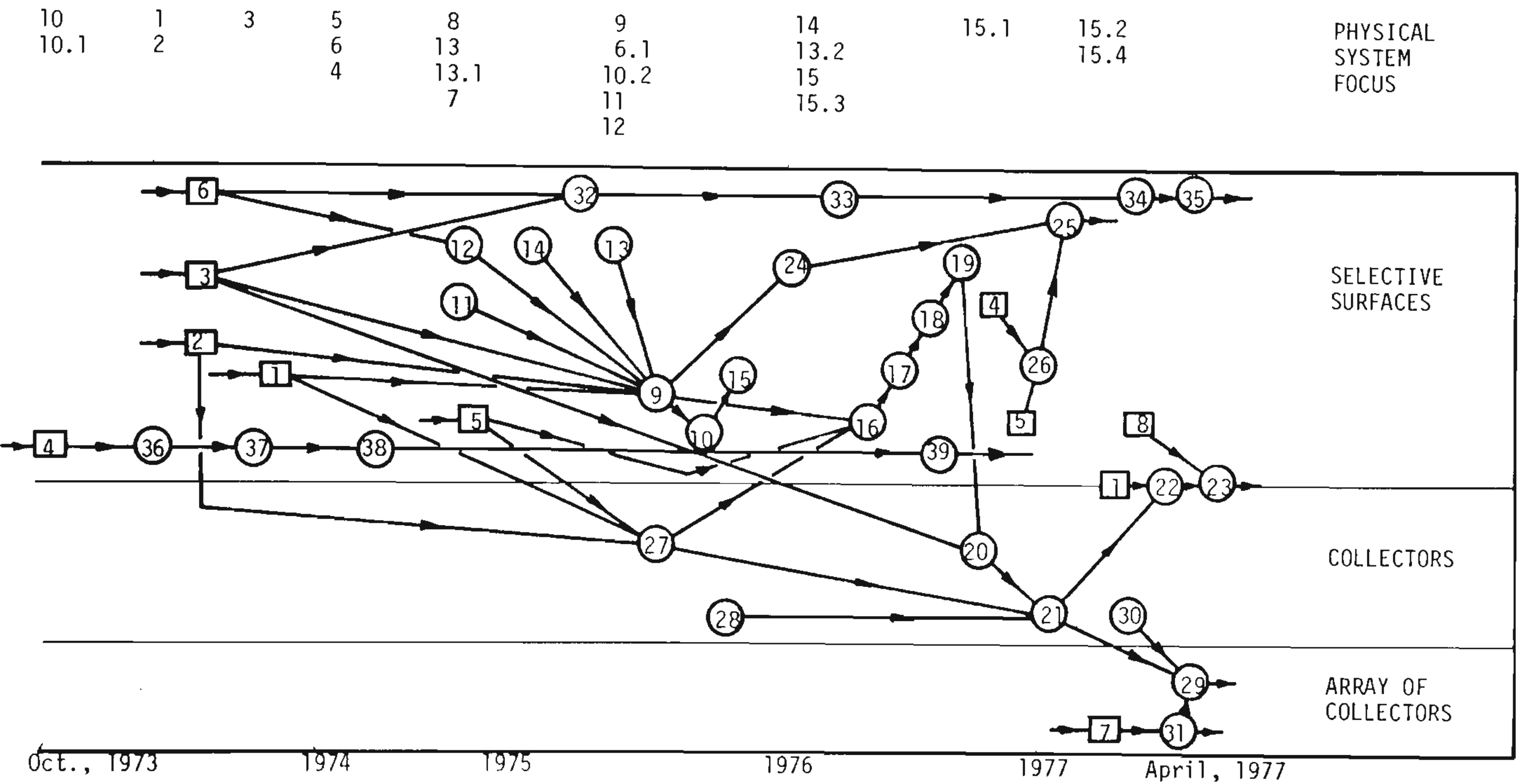
Yours sincerely,

Tom Jagtenberg,
Department of Sociology,
UNIVERSITY OF WOLLONGONG."

Encl.

APPENDIX 4 (cont.)

FIGURE 3: Flow chart of significant research events in the evolution of the selective surfaces program (Round 2 synthesis).



APPENDIX 4 (cont.)

KEY TO FIGURE 3: Significant research events in the evolution of the selective surfaces program (round 2 synthesis).

NOTE: For greater ease of comprehension this list has been arranged to demonstrate something of a logical development of events. This reconstructed logic is sometimes at the expense of the chronological sequence of events.

PROGRAM MEMBERS AND THEIR ESTABLISHED LINES OF RESEARCH:

1. Superconductivity:--------
2. Phase transfer mechanisms in non-conducting solids:--------
3. Magnetism:--------
4. Metal mesh absorbers (Ph.D research):--------
5. Theoretical research on metal meshes:--------
6. Graded interference layers (B.Sc.Hons. and Ph.D research):--------
7. Refrigeration (Ph.D research):--------
8. Sputtered films (Ph.D research):--------

PARTICULAR EVENTS:

9. Metal blacks were established as promising selective surfaces.
10. Research into the properties of gold and chromium blacks was undertaken.
11. Metal meshes rejected as commecially viable selective surfaces on account of optical performance problems.
12. Emissometer developed.
13. Absorptometer developed.
14. -------- (NML) brings to attention the serendipitous discovery in 1952 of the selective nature of gold blacks.
15. Research into the properties of gold and chromium blacks discontinued because of the higher priorities of other research.
16. Reactive sputtering technique developed for a range of materials.
17. Oxides rejected as selective surfaces because of their instability at high temperatures.
18. Metal carbides established as likely prospects (with the caveat that there are no "magic" carbides.
19. A sputtered iron carbide was successfully deposited.
20. Sputtering chamber constructed.
21. An evacuated, tubular glass collector with a sputtered iron carbide selective surface was constructed.
22. Problems with the mass production of carbides investigated.
23. The high rate deposition of sputtered surfaces was investigated.
24. Cermets established as surface candidates.
25. Theoretical advances in cermet theory: the traditional approach in predicting optical constants from the Maxwell-Garnett formula is found to be in error in some cases. An alternative "exact" method using a technique invented by Lord Rayleigh is developed.

APPENDIX 4 (cont.)

KEY TO FIGURE 3 (cont.)

26. Disagreements with a number of workers in the field about the correct theoretical treatment of cermets.
27. Flat plate system rejected.
28. It was discovered that Owens Illinois (US) had independently developed an evacuated glass tubular glass collector. The possibility of using these collectors as the basis for the program's selective surface system remains as an option.
29. An array of collectors is assembled and demonstrated to Neville Wran and the press.
30. A heat exchanger for the collector is developed.
31. Air conditioning device proposed.
32. The grading of the optical properties of a surface was hypothesised to be a potentially viable method of producing a selective surface.
33. Theoretical results suggest that grading had the potential of improving selective surfaces.
34. Grading shown to substantially improve the selectivity of iron carbides.
35. The structure of sputtered carbides was felt to be understood.
36. It was hypothesised that metal meshes might provide a surface with selective properties.
37. Metal meshes developed.
38. It was hypothesised that globs might provide a surface with selective properties.
39. Globs developed.

****

APPENDIX 5: SSP social networks questionnaire

(a) Questionnaire

"The University of Wollongong.

Ref: TJ/MM

28th September, 1978.

Dear

I'm interested in obtaining a somewhat more detailed idea of the social networks relevant to the selective surfaces program over the period October, 1973 to April, 1977 (approximately) - you may recall that this period corresponds with the time span of the events detailed on the various "flow charts" that you've commented on.

Towards this end I would appreciate your assistance in drawing up a list of the people with whom you have had interaction of some relevance to your research over the period under consideration. I'm interested in a fairly wide range of types of relationship and consequently it would help if you could classify these types of relationships using the following code:

Possible type of relationship with you	Code
co-worker	a
regular colleague	b
co-author	c
advisor	d
supervisor	e
student of yours	f
technical assistance to you	g
occasional colleague	h
occasional discussions	i
communication through the literature	j
elite peer	k
representative of funding organisation	l
any other(s) of importance?	m

APPENDIX 5 (cont.)

In addition I need to "locate" these people whom you list -
(a) in some organisation(s), and
(b) with respect to their major scientific interests.

Towards this last end I have listed below the components of the "theoretical landscape" that you've seen before.

Scientific interests of people with whom you've had any of the above types of relationships	Code
Physics	T1
Mechanical Engineering	T2
Solid State Physics	T3
Materials Science	T4
Thin Film Physics	T5
Vacuum Technology	T6
Heat Transfer	T7
Selective Surfaces	T8
Solar Energy Utilisation	T9
Geometric Selective Absorbers:	T10
Metal mesh absorbers	T10.1
Globular metal films	T10.2
Cermet surfaces	T11
Graded Interference Layers	T12
Any others relevant	T13

Regards,

Tom Jagtenberg,
Department of Sociology,
UNIVERSITY OF WOLLONGONG."

APPENDIX 5 (cont.)

SOCIAL NETWORKS QUESTION

I've started this list with a hypothetical example to make things clearer:

Person	Organisational Location	Type(s) of Relationship with you	Major Scientific interests of person
Neville Wran	NSW State Govt.	f, g.	T3, T12.

APPENDIX 6: A reconstruction of the evolution of the SSP based on transcripts of interviews with the program members.

1. The establishment of the solar energy program

A: The group was initially set up by two professors in the School of Physics here. Now, they set it up partly because it was a coming thing, a new project you know, it's obviously going to be funded in the future, it's very topical, gets the University and the Physics School some credit for working in these topical areas - that was one of the reasons. Also I think they wanted the School to have a solid state physics interest which they never had in the past, so they decided to move into solar energy and there was at that stage, a research student here who had an idea of work on metal meshes as selective surfaces - so he had actually enrolled as a Ph.D student to work on selective surfaces, so they said, "well let's work on selective surfaces", and I think it was as simple as that; that's how they made the decision to work in a certain area, then they said, "well we want to employ solid state physicists". So, we were all gathered together and the idea was that we were going to work on selective surfaces - we were all new to solar energy and all new to selective surfaces, you know, I'd done work on magnetism, -------- had done work on neutron defraction and -------- had done work on super conductivities, so there was no connection at all which was a bit of a mistake actually, you know, we could have used some more optical experience.

B: I think it became obvious, probably slowly, but it became obvious to all of us that unless we moved into this . . . into a form of research and development which would lead to a viable solar energy collecting system, then we would not survive as a group, as a research group, mainly because the university has financial problems and this research group would not be maintained unless something very useful, something very important, came out of it, hopefully as quickly as possible. So, we had to think very seriously about what we . . . what sort of collector system we should seriously consider trying to develop and there is a range of options . . . So, to some extent I think we abandoned some of our ideas of just about pure research and started to think seriously about applying our research to hopefully a very potentially useful solar energy collector.

A: Well, we . . . we more or less went into collector from physics and we said that we would make [the collector] from as cheap a material as possible and hope that the production

APPENDIX 6 (cont.)

A: (cont.) at the other end can be cheap, O.K? and that we were gonna end up with an overall cheap collector . . . we thought we'd like to have a selective surface of our own to work on. I was talking to a fellow and he pointed out to us that there was a selective surface that was found in about 1952 by accident by a fellow in the US [who was producing radiation detectors by evaporating gold in an atmosphere that was contaminated with oxygen].

C: I had a background in solid state physics, and all the other people working in our department, with very few esceptions, have a similar background. So we saw it as a problem in solid state physics to develop the selective surface and to use whatever methods we could to develop a surface that was durable, with the right sort of optical properties and (was economically applied) could be economically applied to some tubular substrate.

2. The evolution of the solar energy program

2(a) Technical decisions

A: So then we said, "well, selective surfaces and solar energy; how can we combine the two, what sort of project should we be doing", you know, there are a tremendous number of possible applications of selective surfaces in the solar field and we wanted to be working on an application that was a good application and going somewhere. So we thought about it for some time and we actually thought how can we improve the ordinary flat plate collector and then after a while when we had learnt a little bit more about the theory we realised that that was a bit of a waste of time, not a waste of time really . . . but had gone about as far as it could go and it would be very expensive to improve, and then we said, well we've got these two major heat loss terms and so then we said, well we'll have to evacuate around the collecting element and these decisions were reached over probably about six months, I think, and in a year we started hunting around, and at about the same time we were also working on various possible selective surfaces and learning more about selective surfaces and how do they operate, what sort of physical mechanisms are important in the
so we had more or less two streams on how to improve collectors and how to make surfaces. Then we thought . . . well, we decided we were going to vacuum, now once you decide to go to vacuum you've got to go to tubes because you've got these forces in

APPENDIX 6 (cont.)

A: (cont.) from the atmosphere pushing on the outside of the vessel, so it has to be a tube and the other problem is that you want the vacuum to last twenty odd years, and the only material that has low enough outgasing rate is glass, so we said the thing has to be glass - basically glass - the obvious next step is to make it all glass because that means you can mass produce it very easily . . . it's the type of technology that is well known.

B: But there was a note in some of the papers - particularly in one paper that if you made a mistake in making the gold black you could make it so that it didn't absorb infra red - it became transparent. So this was a selective surface and the first project I undertook was to find out why it [this gold black] behaved like that. So the first project I undertook was to find out why. And it was very fruitful because it suggested - although that individual surface wasn't a good one for selective surface application in solar energy collectors because it wasn't stable at high temperatures - it suggested a lot of others that were. Mixtures of metals and non-metals. The gold black was just a mixture of gold particles and air, air being a non-metal, and although it was totally unsuitable it suggested a whole range of other things which I've since been investigating and some of which have turned out to be very good. And I'm at the stage in that project of working out ways of economically applying them to tubular substrates. So that's one thread that goes through it. Attached onto that thread is the theoretical work which I was doing, I think, when you first came. I think you met -------- --------, we were working down there. That is a theoretical attempt to understand these things in more detail than is presently known.

Obviously one has certain requirements in the collector, one wants a collector system which will last for a very long time - we usually quote twenty-one years for some reason, I'm not quite sure why, as a reasonable lifetime and the point is that certain materials will have better properties than others and finally we will make a choice on a particular material for commercial development but there is, shall we say, another couple of years research involved in improving selective surfaces, testing their properties before one can make this final choice, and this final choice will of course, hopefully, be used in commercial production of our collecting system.

APPENDIX 6 (cont.)

A: So, we've got a cylindrical module and we've got a coke collecting surface in that module which is glass with a selective surface, O.K? So we said, let's sputter our selective surface - now, we'd been working on various methods of putting down selective surfaces up to this stage and so we'd acquainted ourselves with the other techniques that were available, but at this stage we'd made a very definite decision to receive the sputtering because there are a large number of advantages in coating tubes using sputtering, particularly glass tubes; you get excellent adhesion, it's easy to cut long lengths of tubes, it's also a technique - and this was also an argument of mine - it's a technique which is controllable from a physicist's point of view. The other thing was sputtering as you know, it's a physical technique, it's a nice clean technique, there are no horrible waste products like you get from electro plating so it's quite a good way to proceed. It has its disadvantages, the main one being that it's a little bit slow and that's a real disadvantage in mass production.

B: I think we were extremely lucky that we had started work on a particular deposition technique [for the selective surface] which, so it has turned out, has been we think, ideally suited to this type of collector, and I suppose it was more or less an accident - well, it wasn't quite an accident, that we started work on this particular deposition technique, the reason we did start on it was in fact that I had had some experience with this technique in my previous research, so I knew a little bit about it and was able to very quickly construct a sputtering system, as we call it, which enabled us within a few months to start investigating various sputtered selective surfaces.

D: [There are various ways of improving the performance of selective surfaces. One way is by grading the surface to improve their absorptivity by decreasing their reflectivity in the visible region. That . . .] doesn't really interest me that much because it's not a very difficult problem - it's a thing which you can solve using well established principles. A second possibility of improving the absorptivity is to use roughened surfaces and that again is a grating problem - well, I view it as a grating problem let's say, because I'm a grating man. I would say that the simplest type of a rough surface is a perfectly periodic rough surface, right. Other people who are interested in using statistical methods would say that the simplest type of a rough surface is the completely random rough surface.

APPENDIX 6 (cont.)

D: (cont.) I sort of have the attitude in these sorts of problems you should always start from a solution you can solve, a solution which has been established rigorously and then simplify, make assumptions. You see, for me to do a problem rigorously it may take years and that's the drawback with my sort of work. One has faith that one will eventually get an answer, but it can be quite difficult. I think ultimately, my work and the work of people with whom I'm in contact and the work of people who take this field up in response to the obvious need the sort of systematic work will enable us to treat the whole class of structures [of roughened surfaces], but the solar boys can't wait that long, so at the same time I'm forced to attempt to use approximations, approximate solutions to get quick answers. So we make that sort of approximation which I might be able to justify in three years. But, you know, you have to get the results - that's the name of the game, but ultimately I hope that the rigorous theory will catch up, all the problems will have been solved by that time, we'll finally get there.

A: We've probably got a better understanding than any other group in the world on the fundamental side . . . I think the fundamental work has shown us what we knew to make better surfaces and has given us the clues as to how the older surfaces work and I think it's a very important part. This is a criticism of the American programme - they have not funded the fundamental side well enough. Nixon, Ford and Carter have got in there and said, we want results, we want people to be able to see that we're doing something, you know, we need to be re-elected and so that they've actually spent most of their money on things that stick out and people can see, people can appreciate - very applied you see.

D: I think the group derives great benefit from the experimentalists. I think that's one of the great advantages of this place, the group here, one of their great advantages has been that they are in a university framework, they have plenty of interaction with physicists who aren't connected with solar energy, but you know, there's a lot of interdisciplinary input to that group, you know. People talk about their problems over the tea table, you get a lot of ideas, throw them around; I think it's a very good thing here.

APPENDIX 6 (cont.)

2(b) Mediating influences

A: Solar energy in the past has been a very closed community, a very small select community of people have been working on solar energy for the last 20 odd years, O.K? and it hasn't been expanding so we all know each other and it's basically in the Chemical Engineering Department, not in Physics Department because there aren't many in the Physics Department. So with the revival of interest in solar energy and in particular in the Physics Schools, quite a few Physics Schools have an interest now.

B: There has certainly been a tremendous popular interest, but in the scientific community the interest has not been as great as we had expected or hoped. Of course, one can never really tell these days - most of the interest in solar energy is in the scientific, among the scientific community of countries which are facing serious energy problems, the United States and Europe - one can never really tell how much interest they're taking in your work unless you go to talk to them - we publish our work, it is . . . everything that we do is set down in publications for other people to read, they can read it, they can take note of it, they can modify their own work to take into account our discoveries, our progress. I think it's probably the fact that we're so far away from anyone that we hear very little, we've received, I think, surprisingly little interest from the scientific community. People request, write to us and request further information, but so far as I'm concerned anyway, there has not been as much interest as I would have expected and I'm not quite sure of the reason for this . . . a lot of research groups are still doing research more or less for its own sake on a potentially useful subject, that of developing a selective surface but we feel here that unless one pushes the project through to the completion of a viable solar energy collector, then one can not really justify fully working in the field at all, in the field of selective surface development. I think a lot of groups are developing selective surfaces and just hoping that someone else, some other group is going to take up their developments and apply them on a far larger scale in a collective system of some kind. So, we certainly have competitors, but we feel that we are ahead of them only because we have developed means of coating the large area which is involved in a solar collector, and we have these solar collectors now that we can fully test the capability of. There are several very large industries in the United States

APPENDIX 6 (cont.)

B: (cont.) which are producing very similar types of evacuated collectors and we know very little about what they're doing because we think they take the very sensible approach in some ways of keeping their research and development top secret - they are in the game to make money and the only way to make money in a highly competitive field such as this is to keep your research and development fairly quiet - of course, ultimately, one would release, hopefully release, a good product onto the market and hopefully this will be better than anybody else's but one will only make this product better by keeping ahead of one's competitors and so it is necessary to keep your research secret. We have had no choice not to publish all our research simply because this has been the only way that our research group can survive, have been able to survive. We're not part of a multi-million dollar industry, glass industry, or anything of that kind, which can hope to make money, which can hope to invest millions in the hope of obtaining multi-millions - our budget has been trivial and we can only survive by publishing work and hence, proving to university authorities more than anyone that we are doing useful work - we must publish or perish, the old maxim here.

A: One or two companies overseas are now producing evacuated collectors so they are obviously competing [with us] but they're actually producing the things for a market and you just can't find out what the bloody hell the research is about, you know, they just won't tell you and there's no way you can find out, so from a scientific point of view - in fact, I think you'd have to be careful to distinguish the scientific and the commercial competition power - this is going to be one of the worries in the future actually, just how we publish work.

We have agreed . . . there are sorts of agreements that if you're doing work of a commercial nature you agree not to publish it for a year, O.K., but I think that that's not enough for a lot of these people, a lot of the commercial people - they'd like a longer rate. The trouble is that people like myself, -------- and -------- we're judged by publication, you know that, not by the fact that some company or other made a fortune by selling your products. You get also judged by patents, and you can get patents which are nearly as good as publications so I think that may be a way out . . .

C: Another thing that was attached on to the sputtering project [was] the development of a long sputtering chamber, to which three of us contributed: --------, myself and --------. And that's ended up as a patentable machine. Certain aspects of it are

APPENDIX 6 (cont.)

C: (cont.) patentable although the technique isn't new. The application to long substrates is new, in tubular substrates. And, um, so that's more or less been brought to a successful research conclusions, although development has yet to carry on.

D: The big trouble is the conflict between doing physics and thus enhancing your professional reputation and doing the routine engineering and the material science work necessary to get a collector on the market. We have a goal which is to make a marketable heat selective surface, but the younger man without positions must also attempt to do good physics to keep their publication record going. Despite the great breakthrough or whatever you would like to call it, there don't come very many publications in scientific or physical journals just on selective surfaces, I mean, you get one for the sputtering rig and about one for the carbide, the iron carbide, and that's it, so -------- and I have got involved in the theory of cermets and we have been working on that together for about 9 months now and that work is eminently publishable, so that's one of the interesting pressures. You see, the job market in physics is very bad at the moment so it's not just enough for -------- and -------- to develop a good selective surface, they also have to prove that they are good physicists in order that they may apply for lecturing jobs when such jobs appear. In order to get them they not only have to have a reputation because of the selective surfaces work, they have to have an adequate publication list and it's perhaps a bit of a tightrope for them. If you had a few lectureships and a professorship you could probably get the whole group . . . not that it would do me much good at the moment, because the university has the rights to the carbide in collaboration with the scientists who developed it.

B: [Furthermore, in physics research it is often] simply a matter of changing one's field or giving up research altogether - it's simply a phenomenon which . . . it's a recent phenomenon which has resulted from the extreme shortage of jobs in pure research these days, both in universities and in institutions such as the CSIRO here, but one has very little option these days, one is extremely lucky in fact if one can find a job in precisely the research field that you are interested in, so if one seriously wants to remain in research one is often forced to attempt anyway, a move into a completely different research field. I think that these days it is extremely difficult to justify perhaps 90% of research which is done in universities and in research institutions - there are so many applied problems, so many problems which should be solved for the well being of everybody. Well, as I say,

APPENDIX 6 (cont.)

B: (cont.) I think this has been overdone - the amount of interest and financial support for pure research has been overdone at the expense of applied research. The pendulum is swinging the other way now. If one has any . . . is going to have any success at all these days in attracting research funds, then I think one's interests or aims must be far more applied. And right beside these days are the economic problems everywhere in the world.

3. The future

B: I think it'll be successful, yeah, and I think one of the main problems is that it's going to be a very competitive industry; there's gonna be a lot of people in it, because it has tremendous potential . . . you know, millions of dollars . . . potential.

A: One of the problems; we just don't have the market. Next, we obviously want to scale up mass production so we'll be working on largely, you know, ways of coping with large numbers of tubes and various features of that.

B: [In the future] I think that in some ways we may work ourselves out of a job as far as the . . . as far as the applied research is concerned anyway, I think very likely that in three years anyway we will have probably 95% of the problem solved as far as the construction of a perfectly viable, and probably an extremely high quality solar energy collector. The problems which remain after then at that time I think, will be mainly engineering problems which are not really our area, and in some ways not terribly interested in solving ourselves.

So from there on we can only hope that the group will survive, probably in the field of materials research. From there on I think we will have to look at other problems and almost diversify our research aims a little bit, but I think the group will best survive if we do concentrate on materials research because there is always scope for new materials for new applications, for example, one is always looking at new protective coatings to prevent corrosion or to inhibit corrosion, in this sort of field there is a great interest for architecture and any sort of equipment design, so I think we do have a future there; also in the future we will, because of our past history in various pure research

APPENDIX 6 (cont.)

B: (cont.) problems . . . some of us anyway, would like to return and devote some of our time and effort back to pure research and I would hope that we will gain in the future more time to devote some of our activity anyway, to just pure research which interests us, after all this is . . . does seem to be one of the main aims of research in universities, not terribly applied research, but pure research, so it's no reason why we shouldn't diversify our activities and move from applied research more to a field of pure research again.

D: I hope that the success of the group enhances the employment prospects of the members. I don't know whether we will get a selective surface manufacturing industry on a large scale [in Australia] . . . I would hope so. If so, perhaps the members of the group would get into that.

APPENDIX 7: Levels of achievement of the goals of the SSP and rates of progress towards the goals.

Goal	Level of achievement of goals (0-10 scale)			
	January, 1974		April, 1977	
	Level	Mean Deviation (MD)[1]	Level	MD[1]
1	4.6	2.4	9.0	1.0
2	3.0	1.8	9.0	1.0
3	3.6	2.2	9.6	0.8
4	3.0	2.0	9.0	1.0
5	3.0	2.5	10.0	0
6	1.0	1.5	8.6	0.9
6.1	1.0	1.5	8.6	0.9
7	1.6	2.4	8.6	2.5
8			4.6	0.8
9			5.0	2.0
10	4.0	1.0	7.0	2.0
10.1	4.0	1.8	7.0	2.0
10.2	2.0	2.5	8.0	2.3
11			6.6	1.8
12			8.0	1.0
13	1.6	2.3	7.0	1.5
13.1			8.0	1.0
14			8.6	1.0
14.1			8.0	1.5
15			1.6	1.5
15.1			4.0	2.0
15.2			6.0	1.0
15.3			6.6	0.6
15.4			4.0	2.0

APPENDIX 7 (cont.)

Goal	Level of achievement of goals September, 1978		Rate of progress[2] April 75-Sep. 78 (units per month)	Average rate of progress, $\bar{R}$ (units per month)
	Level	MD[1]		
1	9.6	0.8	41×10^{-3}	
2	10.0	0	54	$\bar{R}_{1-5} = 5 \times 10^{-2}$ units/month
3	10.0	0	54	
4	10.0	0	54	
5	10.0	0	62	
6	9.0	1.0	68	$\bar{R}_{6-15.4} = 5 \times 10^{-2}$ units/month
6.1	9.0	1.0	68	
7	9.0	1.5	62	
8	6.0	1.0	41	
9	6.0	2.0	45	
10	7.6	1.8	27	
10.1	7.6	1.5	21	
10.2	8.0	2.0	54	
11	8.0	1.0	59	
12	9.6	0.8	71	
13	8.0	1.0	48	
13.1	9.0	1.0	71	
14	9.0	0.8	77	
14.1	9.6	1.0	71	
15	3.6	1.8	14	
15.1	7.6	2.0	36	
15.2	7.0	1.0	54	
15.3	7.6	0.8	59	
15.4	5.0	1.5	36	

APPENDIX 7 (cont.)

1. The mean deviations are for the first round responses.

2. These rates have been calculated, for the sake of consistency with the rates calculated for the DOP, over a period which extends 16 months beyond the termination of events on the SSP flow diagram. Also, for the sake of comparison, it has been assumed that the levels of achievement for goals that entered consideration after January 1974, were 0 in January 1974.

   Note: Average levels of achievement ($\overline{AT}$) of higher level goals and more technical goals at the cut off point of the in depth analysis (April 1977) are:

   $\overline{AT}_{1-5}$ (April, 1977) = 9.3

   $\overline{AT}_{6-15.4}$ (April, 1977) = 6.6.

APPENDIX 8: Goal orientation of the publications of members of the SSP in 1976 and 1977. +

Goal	Number of publications directed towards particular goals *	Number of authors involved	
		Program members	Non-members
1	2	2	0
2	3	2	0
3	2	2	0
4	0	-	-
5	2	2	0
6	6	2	1
6.1	2	1	0
7	2	1	0
8	3	3	0
9	0	-	-
10	8	2	5
10.1	8	2	5
10.2	4	4	3
11	14	5	2
12	6	5	3
13	7	3	0
13.1	3	3	0
14	3	3	1
14.1	3	3	0
15	0	-	-
15.1	0	-	-
15.2	1	0	2
15.3	4	3	1
15.4	0	-	-

* The sum of the numbers in the first column (83) does not equal the total number of publications of the group, since individual publications were often felt to be directed towards more than one goal. The level of goal specificity of the publications is listed in Appendix 9, which follows.

APPENDIX 8 (cont.)

\+ Program members were asked to match a list of the group's publications against the list of research goals on the basis of goals that were highly influential in accounting for the orientation of the publication. Because there were differences of opinion I have included all the papers indicated by the individual scientists. These numbers are then maximum numbers - the numbers of papers listed by individuals was invariably smaller. This difference does not detract from the interpretation given in the text, however.

("Publication" here means refereed journal articles or conference presentations).

APPENDIX 9: Specificity of the SSP's publication record in the period 1976-1977.

Possible number of different goals towards which a publication was said to be directed	Numbers of publications directed towards particular numbers of goals
1	9
2	10
3	3
4	2
5	0
6	0
7	2
8	2

Total number of publications in 1977 and 1978 * = 28

* This figure excludes similar papers presented in different contexts, for example, a conference paper subsequently published in a journal is only counted once.

APPENDIX 10: Location of the goals of two members of the SSP in the theoretical landscape.

	Theoretical components													
Goal	1	2	3	4	5	6	7	8	9	10	10.1	10.2	11	12
1	x		x#	x				x					x	x
2				x				x#	x					
3			x	x	x			x#	x	x#	x	x	x	x
4				x		#		x	x					
5				#	#			x	x	x	x	x	x	x
6				x				x#					x#	x#
6.1				x#				x					x	x
7									x#					
8		#			x	x#	x		x#					
9	x	#												
10								#		x#				
10.1								#		#	x			
10.2								#		#		x		
11								#					x#	
12								#						x#
13					x	#		x					x	x
13.1				#	x	#								
13.2					x	#								
14				x	x	x#	x	x	x					
15		#												
15.1		#					#		x					
15.2	#						x#		x					
15.3				x#	x	x	x	x						
15.4		#							x					

KEY: x: Program member 1
#: Program member 2

APPENDIX 11: Priorities and autonomy indices for the SSP.

Goal	Average priority for core group (0-10 scale)[1]	Mean deviation (MD)[2]	Autonomy Index for core group	MD[2]
1	6.1	3.6	-3.0	2.0
2	6.7	3.2	-3.0	2.0
3	4.2	3.7	-3.3	2.2
4	3.4	1.6	-2.3	1.4
5	4.7	2.5	-3.3	2.2
6	3.5	1.9	-0.7	0.9
6.1	3.5	1.6	-0.7	0.9
7	4.4	1.8	-2.0	2.0
8	6.1	1.7	-0.3	0.4
9	3.4	3.5	0.3	1.4
10	4.6	4.6	0	0
10.1	4.8	4.8	-0.3	0.4
10.2	5.0	5.0	-0.3	0.4
11	2.8	0.8	0.7	0.9
12	1.7	1.7	-2.0	1.3
13	4.1	1.7	0.3	0.4
13.1	3.8	1.0	-1.7	2.3
14	3.3	1.9	-0.3	0.4
14.1	2.7	2.0	0	0
15	3.0	3.0	0.3	0.4
15.1	3.3	3.3	-0.3	0.4
15.2	3.5	3.5	-1.7	0.4
15.3	4.0	2.3	-0.7	0.9
15.4	3.9	3.9	0.3	0.4

2 The mean deviations derived from first round responses.

1 These figures have been rescaled from a rank order scale to a 0 (low) to 10 (high) scale.

APPENDIX 12: First round summary and questionnaire administered to members of the DOP.

Letter:

"The University of Wollongong
Department of Sociology.

Ref: TJ/MM

25th March, 1978.

Dear

I have been doing sociological research on some clinical pharmacological research that you have had some involvement with. The research I'm referring to was broadly focused on dopamine and octopamine and occurred in the Clinical Pharmacology Department of the University of New South Wales (in St. Vincent's Hospital) over the period January, 1965 to October, 1976 (approximately).

It would be of great assistance to me if you could look over some of the material that I have enclosed and make some comment about the accuracy of my results to date, particularly where I have dealt with events that concern you. It would be even more useful if you could attempt the questionnaire that I've included.

Hoping to hear from you,

Yours sincerely,

Tom Jagtenberg,
Department of Sociology,
UNIVERSITY OF WOLLONGONG."

Encl.

APPENDIX 12 (cont.)

FIRST ROUND RESULTS AND QUESTIONS.

The following results are presented for discussion:

Figure 1: The theoretical landscape of your research program.

Figure 2: A list of theoretical and technical goals that affected the direction of your program.

Figure 3: A flow chart of significant research events on your program.

Figure 4: A summary analysis of the goals of your program.

These results should be read in conjunction with the questions as an explanation of some of the key terms is provided with the questions.

APPENDIX 12 (cont.)

FIGURE 1: Theoretical landscape of the dopamine/octopamine program.

Level of theoretical landscape	Theoretical components and their professional orientation*		
	Scientific		Medical
Discipline	T1	Clinical pharmacology	
	T2 Biology		
	T3		Medicine
	T4 Biochemistry		
Specialty	T5	Neuropharmacology	
Program	T6 Neural transmission theory, particularly mechanisms of transmitter reception in the brains of: (a) invertebrates (viz: molluscs) (b) vertebrates (viz: rats) (c) man		
	T7 Dopamine theory of schizophrenia		
	T8 Dopamine/octopamine theory of schizophrenia		
	T9 Noradrenalin theory of schizophrenia		
	T10 Serotonin theory of schizophrenia		

* The components have been listed under their most influential professions. Where the components are oriented to both the professions they have been listed in the centre of the Figure.

FIGURE 1 is intended to be a general description of the "theoretical landscape" of the dopamine/octopamine program as perceived by the program members. The term "theoretical landscape" is meant to cover the scientific law, models, examples of theoretical applications and "facts" that are directly referred to and which provide a working background for the research conducted on the dopamine/octopamine program. It is assumed that at the level of generality indicated the "landscape" will be similarly perceived by all program members. If this is not the case, please indicate where the differences lie.

This notion of theoretical landscape is obviously broad in scope, but I am attempting to distinguish, as far as possible,

APPENDIX 12 (cont.) [Figure 1 cont.]

between those theoretical aspects that you are conscious of using and referring to and a whole structure of knowledge which could extend through many disciplines and many years of education. A further distinction is being drawn here between "theory" and the more immediate "practical" concerns of your program. I have attempted to express these latter concerns in the goals listed in Figure 2.

Q1: Any corrections, additions or general comments?

APPENDIX 12 (cont.)

FIGURE 2: A list of theoretical and technical goals that affected the direction of the dopamine/octopamine program (up to October 1976).

Level of theoretical and technical goals	Professional Orientation*		
	Scientific		Medical
A. THEORETICAL GOALS			
Discipline	G1	Investigate the physiological effects of a broad range of drugs	
Specialty	G2 Develop general models relating to: (a) brain function (b) drug action in the brain		
Program	G3		Develop a drug for the cure of schizophrenia
	G4	Investigate an interesting chemical (ℓ-dopa) and its metabolites	
	G5	Elucidate dopaminergic and octopaminergic mechanisms and their role in schizophrenia	
	G6 Elucidate the biochemistry of a series of dopamine related chemical compounds that are involved in chemical transmission systems in the human brain		
	G7		Elucidate the role of dopamine and octopamine in the biochemical mechanisms associated with schizophrenia

* The components have been listed under their most influential professions. Where the components are oriented to both the professions they have been listed in the centre of the Figure.

APPENDIX 12 (cont.) [Figure 2 cont.]

Level of theoretical and technical goals	Professional Orientation*	
	Scientific	Medical
Program (cont.)	G8 Develop a model of the pre- and post-synaptic mechanisms of dopamine and octopamine related neural transmission systems in the brains of: (a) molluscs (b) animals (viz: rats) (c) man G9 Specific research tasks following on from G4 (these research tasks were formally expressed in a recent - April 1976 - NHMRC research grant application): (a) Precisely to specify the structural requirements for agonist and antagonist activity at the discrete post-synaptic receptors for dopamine and octopamine (b) To define the nature of pre-synaptic mechanisms for the uptake, storage and release of octopamine and to compare these mechanisms with those known to be involved with other neurotransmitters. (c) To define the activity of hallucinogenic and anti-psychotic drugs at the specific pre- and post-synaptic mechanisms for dopamine and octopamine and to study the effects of other psychotic active drugs on these mechanisms. (d) To design, synthesise and test compounds which may function as specific octopamine receptor blocking agents. (e) To study the activities of compounds affecting octopaminergic mechanisms on the behaviour of animals.	

APPENDIX 12 (cont.) [Figure 2 cont.]

Level of theoretical and technical goals	Professional Orientation*	
	Scientific	Medical
Program (cont.)	G9 (f) To study the effects of compounds known to affect octopaminergic mechanisms on the storage, distribution and metabolism of octopamine.	
B. TECHNICAL GOALS		
Program	G10 Develop chemical assays for: (a) dopamine (b) octopamine G11 Synthesise radioactive neuro-transmitters	

The next questions relate to FIGURE 2, the list of research goals. It has been attempted to order this list in an approximate order of increasing "closeness" to day-to-day research. As before with the "landscape" in Figure 1, it is assumed that these goals are mutually perceived. If this is not the case, please indicate where any differences lie.

Q2: (i) Any corrections, additions, or general comments?

(ii) Have any of these goals changed since then?

Q3: Which people were involved in the formulation of each of the goals? Where possible, rank the influence of the people on these lists 0 - 5 where the numbers have the following meanings -

0 or 1) low

2 or 3) medium

4 or 5) high influence

APPENDIX 12 (cont.) [Figure 2 cont.]

Q4: Have any of the goals been achieved?

How would you rate progress towards the various goals - both at October 1976 (or thereabouts) and now:

0 - 5, with the correspondencies

0 or)
1) low

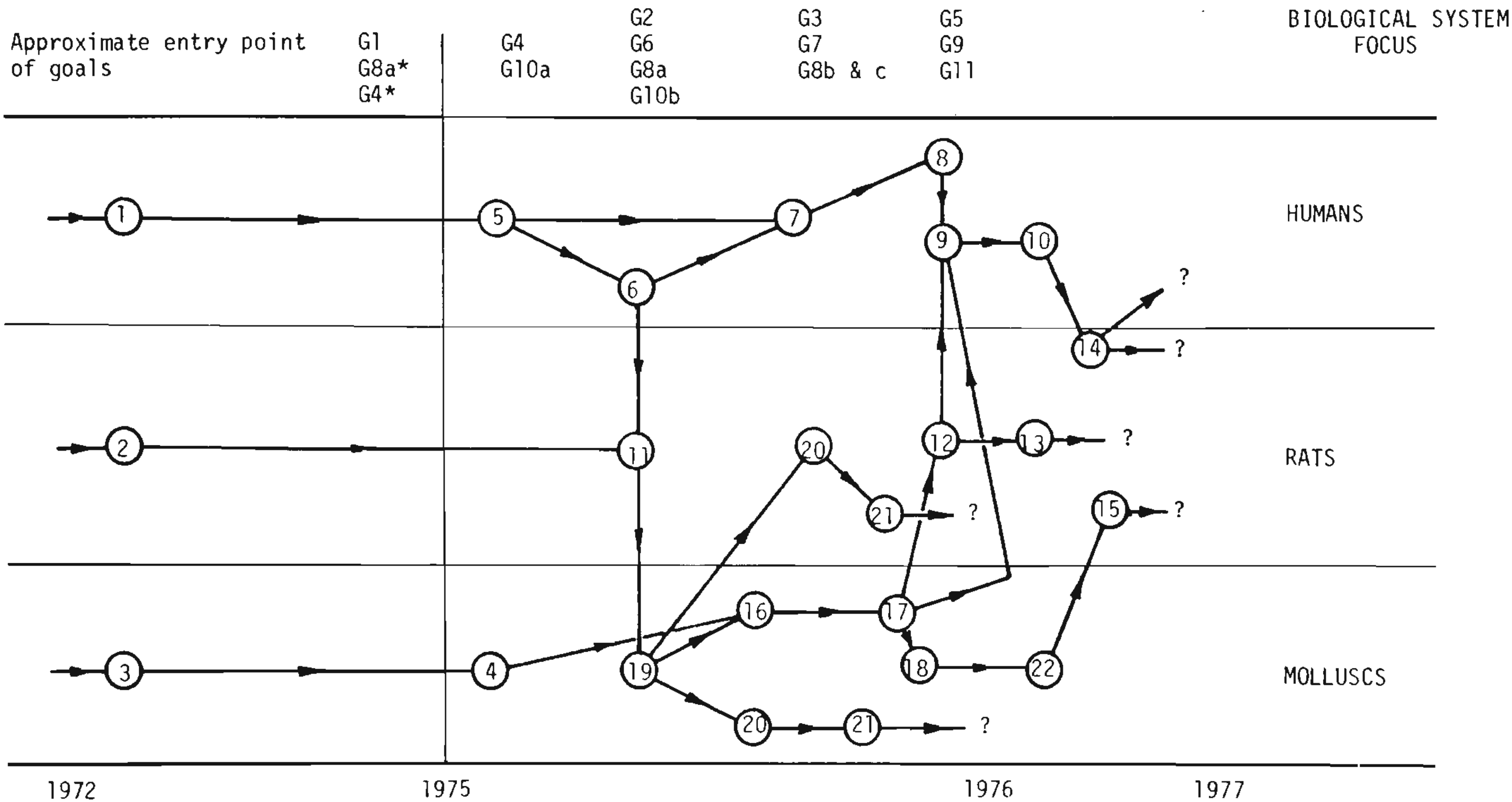
2 or)
3) medium

4 or)
5) high progress

FIGURE 3 is a flow chart of significant research events in the evolution of the dopamine/octopamine program. The events being considered here are in the class of important observations, hypotheses, ideas, strategies, theoretical developments, technical developments, etc. The key to the numbered events is on [pp.541 and 542]. The goals referred to on the top of the diagram are the same as in Figure 2.

APPENDIX 12 (cont.)

FIGURE 3: Flow chart of significant research events in the evolution of the dopamine/octopamine program.



* These goals had a very different context before the establishment of the dopamine/octopamine program.

APPENDIX 12 (cont.) [Figure 3 cont.]

KEY TO FIGURE 3: Significant research events in the evolution of the dopamine/octopamine program. (Note: this list is not in chronological order).

1. Clinical research on Parkinson's Disease.

2. Research on drug absorption/protein binding.

3. Research on dopamine receptors in molluscan tissue.

4. Metachlorpramide demonstrated to be a dopamine antagonist.

5. Schizophrenic side effects observed after treatment with ℓ-dopa.

6. Parkinsonism observed to be a side effect of some anti-psychotic drugs.

7. Dopamine hypothesised to be significant in schizophrenia.

8. Dopamine and octopamine hypothesised to be significant in schizophrenia.

9. Multiple sites hypothesised for the reception of dopamine and octopamine.

10. The hypothesised significance of octopamine is supported by evidence gained from a comparison of the action of the drugs Clozapine and Perlopen on human subjects.

11. An investigation of the mechanisms of dopamine reception is undertaken.

12. An investigation for multiple sites for dopamine reception in rat tissue is desired.

13. Technical inadequacies prevent this line of research.

14. Rats demonstrate aggressive (psychotic) behaviour due to high blood concentrations of octopamine.

15. Evidence of specifically dopamine sensitive receptor sites in rat tissue is received from overseas.

16. Certain anti-psychotic drugs do not act as dopamine blockers on particular receptor sites.

17. Multiple sites hypothesised for the reception of dopamine.

APPENDIX 12 (cont.) [Figure 3 cont.]

18. Octopamine is observed to be absorbed by a dopamine receptor.

19. Major technical problems experienced with the working up of dopamine and octopamine assays.

20. Major review and systematisation of present status of research undertaken.

21. Development of chemical assay techniques continued.

22. Evidence gained from a comparison of the action of Clozapine and Perlopen in rat brains suggests that octopamine has its own receptors.

APPENDIX 12 (cont.)

Q5: Any corrections, additions or general comments?

In the next questions a distinction is being made between "active involvement" and "merely influential involvement". Active involvement in research is taken to mean participation in the day-to-day conduct of research at a practical and/or managerial level. This is distinguished from merely influential involvement which is taken to mean involvement which does not extend to practical and/or managerial participation in day-to-day research. Thus, scientists who have contributed only through the literature, or scientists with whom you must communicate about your research would be influential. Strictly speaking, an actively involved scientist would also be influentially involved but not "merely" influentially involved).

Q6: Which program members were actively involved at each event depicted in the diagram?

If possible, rank the members with respect to -

(i) their contribution of labour, and
(ii) their contribution of ideas.

Rank the contributions 0 - 5 where the numbers have the following meaning:

0 or 1) low

2 or 3) medium

4 or 5) high contribution

Q7: Were there any other scientists actively involved in any of the events shown in Figure 3?

If possible, rank the participants with respect to -

(i) their contribution of labour, and
(ii) their contribution of ideas.

Rank the contributions 0 - 5 where the numbers have the following meaning:

APPENDIX 12 (cont.)

Q7:

0 or)
1) low

2 or)
3) medium

4 or)
5) high contribution

Q8: Were there any other people actively involved in any of the events shown in Figure 3?

If possible, rank the participants with respect to -

(i) their contribution of labour, and
(ii) their contribution of ideas.

Rank the contributions 0 - 5 where the numbers have the following meaning:

0 or)
1) low

2 or)
3) medium

4 or)
5) high contribution

Q9: Were there any others who were merely influentially involved in the events shown in Figure 3?

List (a) other program members,
(b) other scientists, and
(c) others.

Where possible, rank the influence of the people on these lists 0 - 5 where the numbers have the following meaning:

0 or)
1) low

2 or)
3) medium

4 or)
5) high influence

APPENDIX 12 (cont.)

Q10: Was any part of the literature particularly significant to events on the diagram?

Please specify.

Q11: What were the major pieces of equipment involved in the various events on the diagram?

Q12: Were there any standard techniques that were frequently used in the various events on the diagram?

The next questions are related to the range of alternatives that surrounded the events shown in FIGURE 3. What is being investigated here is the degree to which the various events shown in the figure were the product of choices that were made between alternative courses of action.

Q13: Was there a range of alternatives (i.e., other courses of action/interpretation) available at the various points on the diagram?

List the alternatives (even if they appear somewhat impractical/fanciful/silly) from the standpoints of -

(i) the way things seemed at the time, and
(ii) the way things appear now.

Q14: Scale the degree of choice that you feel surrounded each of the events shown on the diagram.

Use the same ranking as before, i.e., 0 - 5 with the correspondencies:

0 or 1) low

2 or 3) medium

4 or 5) high choice

APPENDIX 12 (cont.)

Q15: Would you say that any of the events shown on the diagram were serendipitous (or the product of some other serendipitous events)?

Q16: What were the reasons for the particular choice that was actually made?

Q17: Would it have been better to have followed a different path at any point? Why?

Q18: (i) Have any publications resulted from the phase of research shown in the Figure?

(ii) Are there any papers likely to result from that phase of the research?

(iii) Have there been any other products/outcomes? for example, patents, processes, equipment, new funding, staff changes, other constraints . . .?

APPENDIX 12 (cont.)

FIGURE 4: Summary analysis of the goals of the dopamine/ octopamine program. **

Goals - in approx. order of appearance	Theoretical components and other research goals expressed in the goals	Summary explanation of significant social factors contributing to the establishment and evolution of goals
G1	T1 - T5	1. Professional orientation: General medical and scientific obligations must be fulfilled; this goal is an expression of a broad sweep of ongoing research by a variety of scientifically and medically trained workers. 2. Support structure: active pursuit of this goal is a necessary condition for continued funding from the hospital, drug company, university, and other contract sources.
G8a*	T2,T4,T6	1. Professional orientation: The goal was part of the (now) senior research officer's Ph.D work. 2. Support structure: The goal was a sufficient condition for the employment of the (now) senior research officer by an agency of the drug company.
G4*, G4	T1 - T6, G1,G8a*, G10a	1. Professional orientation: The goal was established as a reasonably specific common scientific and medical interest between the (now) senior research officer and the program leader.

** See Figures 1 and 2 for the key to the numbering of the theoretical components and research goals.

APPENDIX 12 (cont.) [Figure 4 cont.]

Goals - in approx. order of appearance	Theoretical components and other research goals expressed in the goals	Summary explanation of significant social factors contributing to the establishment and evolution of goals
		The goal was formalised as one of the major goals of an earlier research program which was concerned with the role of dopamine in Parkinson's Disease and can be considered as a fruitful upshot which has been formalised in a range of specific research tasks (G9). 2. Support structure: The goal was a sufficient condition for the employment of the senior research scientist by the NHMRC to conduct research on the Parkinson's Disease program. 3. Organisation of labour: Initially there was considerable division of labour between the more medically oriented team leader and the more biochemically oriented senior research officer, however the direction of the program now involves less of a distinction between medical and scientific aspects. On the present program the goal is the product of, and pursued from the basis of the specialist interests of the senior research scientist (i.e., molluscan systems) and the program leader (i.e., human biochemistry). The implications of this goal are always subject however, to the overall co-ordination of the team leader.
G10a	T1 - T6, G1, G8a* G4	1. Exchange relations: It was found impossible to replicate an octopamine assay reported in the literature, and consequently it became necessary to develop a new assay.

APPENDIX 12 (cont.) [Figure 4 cont.]

Goals - in approx. order of appearance	Theoretical components and other research goals expressed in the goals	Summary explanation of significant social factors contributing to the establishment and evolution of goals
G2	T1 - T6, G1,G6,G8a, G10a & b	1. Professional orientation: The goal was established as a scientifically fruitful general orientation for clinical pharmacological research. 2. Support structure: The goal was recognised to entail a potentially support winning area of research.
G6	T1 - T6, G1,G2, G8a	-
G8a	T1 - T6, G1,G2, G6,G8a*	1. Professional orientation: The goal is an extension of the senior research officer's Ph.D work, but now in the context of a partially medically oriented program.
G10b	T1 - T6, G1,G2,G6, G8a	-
G3	T1 - T7, G1,G2,G4, G6 - G8,G10	1. Professional orientation: This goal is the ultimate medical legitimator for the program, and is also a highly significant scientific legitimator. 2. Support structure: This goal is the ultimate economic legitimator for the program. If successfully realised there would be a source of royalties for the university and a marketable product for the drug company.

APPENDIX 12 (cont.) [Figure 4 cont.]

Goals - in approx. order of appearance	Theoretical components and other research goals expressed in the goals	Summary explanation of significant social factors contributing to the establishment and evolution of goals
G7	T1 - T7, G1 - G4,G6, G8,G10	-
G8c	T1 - T7, G1 - G4, G6,G7, G8a & b	1. Professional orientation: This goal is a general medical and scientific legitimator of the relevance of the research. 2. Support structure: This goal is a necessary condition for continued support from the NHMRC and a significant factor for the winning of continued support from other scientific circles.
G8b	T1 - T7, G1 - G4,G6, G7,G8a & c	1. Professional orientation: This goal is practically and morally necessary (scientifically and medically) as a first step in research oriented towards humans.
G5	T1 - T7, G1 - G4, G6 - G8,G10, G11	-
G9	T1 - T7, G1 - G8, G10,G11	Support structure: This series of research tasks is a formalised version as presented in an application for support from the NHMRC.
G11	T1 - T7, G1 - G10	-

APPENDIX 13: Second round summary and questionnaires administered to members of the DOP.

Letter: "The University of Wollongong
Department of Sociology.

Ref: TJ/MM

25th May, 1978.

Dear

Here are some of the results of my research to date. These results represent my attempts to establish a consensus about various aspects of the dopamine/octopamine program over the period January, 1975 to October, 1976 (approximately). The results are particularly based on our last discussion, where we talked about my first attempt to present a picture of your joint research over the abovementioned period of time.

I am particularly interested to know whether you differ in your views from the picture I have presented as a "consensus". You will notice that I have provided spaces for you to register any disagreements that you may have.

Apart from the presentation of an updated "flow diagram" for your comments some time in the future, this is probably the last questionnaire you'll receive from me. Therefore, as a relatively "final" attempt to get the picture straight, your comments are particularly valuable this time round.

Yours sincerely,

Tom Jagtenberg,
Department of Sociology,
UNIVERSITY OF WOLLONGONG."

Encls.

APPENDIX 13 (cont.)

DEFINITION OF TERMS.

A. Research Program: Ideally this means a collective enterprise that involves individual researchers working towards shared goals. Of course in practice the level of sharing may not extend to all the goals of all the researchers, or all the elements in the "background" knowledge of individuals, but so long as there exists some mutual co-operation and work towards goals which are part of a loosely (or strongly) co-ordinated research effort, a "program" is considered to exist.

B. Theoretical Landscape: As I defined the term in the last questionnaire, "theoretical landscape" is essentially a structure of theoretical knowledge that provides a working background for researchers working together on a research program. The term is meant to cover the scientific law, models, examples of theoretical applications, and "facts" that are most directly referred to and which provide a working background for the research conducted on a research program. It is assumed that at the level of generality indicated the "landscape" will be similarly perceived by all program members. It is appreciated that individual interpretations may vary but it is taken for granted that the possibility of any level of co-operative research depends on a considerable degree of shared understanding at discipline, specialty and program levels. Where differences in experience, abilities, interests, etc., work against this shared understanding, it is further assumed that the possibility of co-operative work will depend on some degree of submission to someone's authority.

This notion of theoretical landscape is obviously broad in scope, but I am attempting to distinguish, as far as possible, between those theoretical aspects that you are conscious of using and referring to and a whole structure of knowledge which could extend through many disciplines and many years of education. A further distinction is being drawn here between "theory" and the more immediate "practical" concerns of your program. I have attempted to express these latter concerns in the goals listed in Figure 2.

APPENDIX 13 (cont.)

FIGURE 1: Theoretical landscape of the dopamine/octopamine program.

Level of theoretical landscape	Theoretical components and their professional orientation*			
		Scientific		Medical
Discipline	T1		Clinical pharmacology	
	T2	Biology		
	T3			Medicine
	T4	Biochemistry		
Specialty	T5		Neuropharmacology	
	T6		Neurophysiology	
Program	T7	Neural transmission theory, particularly the role of catecholamines and phenolamines as neurotransmitters in the nervous systems of: (a) invertebrates (viz: molluscs) (b) vertebrates (viz: rats) (c) man		
	T8	Dopamine theory of schizophrenia		
	T9	Dopamine/octopamine theory of schizophrenia		
	T10	Noradrenalin theory of schizophrenia		
	T11	Serotonin theory of schizophrenia		

* The components have been listed under their most effective professions. Where the components are oriented to both the professions, they have been listed in the centre of the Figure.

APPENDIX 13 (cont.)

FIGURE 2: A list of theoretical and technical goals that affected the direction of the dopamine/octopamine program (up to October, 1976).

Level of theoretical and technical goals	Goal	Professional orientation* — Scientific	Professional orientation* — Scientific and Medical	Professional orientation* — Medical	Approximate date of emergence of the goal **
A. THEORETICAL GOALS					
Discipline	G1		To investigate the physiological effects of a broad range of drugs on man and other animals		1974
Specialty	G2	To develop general models relating to: (a) brain function (b) drug action in the brain			April, 1975
	G3	To understand the biochemical basis of psychiatric disease			July, 1975
Program	G4			(a) To develop a drug for the control of schizophrenia/psychosis	July, 1975
				(b) To develop screening processes for antipsychotic drugs	July, 1975

* The components have been listed under their most influential professions. Where the components are oriented to both the professions, they have been listed in the centre of the Figure.

** That is, emergence of the goal as significant in accounting for the direction of research.

<u>APPENDIX 13</u> (cont.) [Figure 2 cont.]

Level of theoretical and technical goals	Professional orientation*				Approximate date of emergence of the goal **
		Scientific		Medical	
Program (cont.)	G5		To investigate an interesting chemical (ℓ-dopa) and its metabolites with particular emphasis on the anti-Parkinson and psychomimetic properties of ℓ-dopa		January, 1975
	G6		To elucidate dopaminergic and octopaminergic mechanisms and their role in schizophrenia		October, 1975
	G7	To elucidate the biochemistry of a series of dopamine related chemical compounds that are involved in chemical transmission systems in the rat brain (and ultimately in the human brain)			April, 1975
	G8			To elucidate the role of dopamine and octopamine in the biochemical mechanisms associated with schizophrenia/psychosis	July, 1975

APPENDIX 13 (cont.) [Figure 2 cont.]

Level of theoretical and technical goals	Professional orientation* — Scientific	Professional orientation* — Medical	Approximate date of emergence of the goal **
Program (cont.)	G9 To develop a model of the pre- and post-synaptic mechanisms of dopamine and octopamine related neural transmission systems in the brains of:		
	(a) molluscs		April, 1975
	(b) animals (viz: rats)		July, 1975
	(c) man		July, 1975
	G10 Specific research tasks following on from G4 (these research tasks were formally expressed in a recent - April 1976 - NHMRC research grant application. The application was originally written in May 1975):		October, 1975
	(a) Precisely to specify the structural requirements for agonist and antagonist activity at the discrete post-synaptic receptors for dopamine and octopamine.		
	(b) To define the nature of pre-synaptic mechanisms for the uptake, storage and release of octopamine and to compare these mechanisms with those known to be involved with other neurotransmitters.		

APPENDIX 13 (cont.) [Figure 2 cont.]

Level of theoretical and technical goals	Professional orientation*		Approximate date of emergence of the goal **
	Scientific	Medical	
Program (cont.)	(c) To define the activity of hallucinogenic and anti-psychotic drugs at the specific pre- and post-synaptic mechanisms for dopamine and octopamine and to study the effects of other psychomimetic drugs on these mechanisms. (d) To design, synthesise and test compounds which may function as specific octopamine receptor blocking agents. (e) To study the activities of compounds affecting octopaminergic mechanisms on the behaviour of animals. (f) To study the effects of compounds known to affect octopaminergic mechanisms on the storage, distribution and metabolism of octopamine.		

APPENDIX 13 (cont.) [Figure 2 cont.]

Level of theoretical and technical goals	Professional orientation*		Approximate date of emergence of the goal **
	Scientific	Medical	
B. TECHNICAL GOALS			
Program	G11 To develop chemical assays for:		
	(a) dopamine		January, 1975
	(b) octopamine		April, 1975
	G12 To synthesise deuterated neuro-transmitters and related metabolites.		October, 1975

APPENDIX 13 (cont.)

RESULTS AND QUESTIONS.

1. On the basis of my last round of discussions there appears to be a high level of consensus about the "theoretical landscape" of the dopamine/octopamine program as perceived by program members.

   Do you have any disagreements with the picture presented in Figure 1? Place your comments on the Figure, if that's more convenient.

2. On the basis of our last round of discussions there also appears to be a high level of consensus about the major research goals that were effective on the program over the period January, 1975 to October, 1976 (approximately).

   However, I do not have a clear impression of the relative importance of these goals. Towards this end I have presented in Figure 2 a list of research goals that appears to reflect a consensus amongst --------, --------, --------, and -------- (that is, amongst all those whom I have had discussions with about the goals). The list is substantially the same as the list you have already seen and commented upon; the small amendments are a product of the new information I gathered in the last round of interviewing.

   Q.2.1: Do you still agree with the formulation of the goals as presented in Figure 2? That is, were these goals a reality as far as you were concerned, and are they a relatively complete and accurate expression of the research ends towards which you were moving, and in most cases towards which you still are moving?

   Place your comments on the Figure if that's more convenient.

   Q.2.2: Is it possible for you to list the goals in an order of priority, as far as you were concerned, and as far as the group was concerned? Rank the goals numerically: 1, 2, 3, . . . n.

   If there were any major changes in priorities over the period under study, please indicate where.

APPENDIX 13 (cont.)

Goals	Personal Priority	Priority for group (if different)
G1		
G2 (a)		
(b)		
G3		
G4 (a)		
(b)		
G5		
G6		
G7		
G8		
G9 (a)		
(b)		
(c)		
G10 (a)		
(b)		
(c)		
(d)		
(e)		
(f)		
G11 (a)		
(b)		
G12		

Q.2.3: Which were the goals that most affected your work? Here I'm interested in the relative extents to which you were involved in working towards some goals rather than others. "Involvement" is meant to include managerial and/or "hands on" work. Scale your involvement on a 0 - 5 basis where the numbers have the following meanings:

0 or 1 — low

2 or 3 — medium

4 or 5 — high involvement

APPENDIX 13 (cont.)

If there were any changes over the period, please indicate where.

Goal	Level of Involvement (0 - 5)
G1	
G2 (a)	
(b)	
G3	
G4 (a)	
(b)	
G5	
G6	
G7	
G8	
G9 (a)	
(b)	
(c)	
G10 (a)	
(b)	
(c)	
(d)	
(e)	
(f)	
G11 (a)	
(b)	
G12	

Q.2.4: How many days/week on average would you say you were engaged on dopamine/octopamine relevent research in the period under consideration?

Q.2.5: How would you classify the type of your involvement (on average):

i. managerial and/or experimental?

ii. creative and/or routinely technical?

iii. any other way?

APPENDIX 13 (cont.)

Q.2.6: Have there been any new research goals that have emerged as important to the direction of the dopamine/octopamine related research since October 1976? Have any of the priorities changed because of these new goals?

Goal	Personal Priority now (May, 1978)	Priority for group now
G1		
G2 (a)		
(b)		
G3		
G4 (a)		
(b)		
G5		
G6		
G7		
G8		
G9 (a)		
(b)		
(c)		
G10 (a)		
(b)		
(c)		
(d)		
(e)		
(f)		
G11 (a)		
(b)		
G12		
+ new goals (give approx. date of emergence)		
+		
+		

APPENDIX 13 (cont.)

Q.2.7: Presented below are the average responses to the last round question about progress towards goals.

(i) If you have a different view of the progress made, please indicate where your differences lie.

(ii) Could you update the account of progress made to the present date?

(iii) Have there been any publications or other products that have been the result of work towards any of the goals in particular, or perhaps towards a group of goals? I have enclosed a list of publications that might be relevant. Please list the appropriate number of the publication beside the appropriate goal/s. The scale used was:

0 or 1) low

2 or 3) medium

4 or 5) high progress

Note: A rating of 5 is taken to mean that the goal has been successfully achieved.

	Progress towards goals			
Goal	Oct. 76	Feb. 78	May 78	Publications or other products?
G1	2	3		
G2 (a)	2.5	4		
(b)	2.5	4		
G3	1.5	2		
G4 (a)	1.5	2		
(b)	1.5	3		
G5	2.5	4		
G6	1.5	2		
G7	2.5	4		
G8	2	3		

APPENDIX 13 (cont.)

Goal	Oct. 76	Progress towards goals Feb. 78	May 78	Publications or other products?
G9 (a)	2.5	4		
(b)	2.5	4		
(c)	1	2		
G10 (a)	5	5		
(b)	2	4		
(c)	2	3		
(d)	3	3		
(e)	2	3.5		
(f)	2	4		
G11 (a)	2	5		
(b)	2	5		
G12	0	5		
+ new goals				
+				
+				

Q.2.8: Listed below are the average responses to the last round question about the influence of various people on the formation of the goals. If you have a different memory of events please indicate where your differences lie. The scale used was:

0 or 1) low

2 or 3) medium

4 or 5) high influence

APPENDIX 13 (cont.)

Influence of people on the formation of goals

Goals	--------	--------	--------	--------
G1	5	0	0	0
G2 (a)	5	0	0	0
(b)	5	0	0	0
G3	5	0	0	0
G4 (a)	5	0	4.5	0
(b)	5	0	0	0
G5	0	0	0	0
G6	4	5	0	0
G7	4	5	0	0
G8	5	3	0	0
G9 (a)	4.5	3.5	0	3
(b)	3.5	4	0	0
(c)	4	3.5	0	0
G10 (a)	3.5	5	0	0
(b)	4.5	4.5	0	2
(c)	4	5	0	3
(d)	5	4	0	0
(e)	4	5	0	0
(f)	5	4	0	0
G11 (a)	2	2	5	0
(b)	2	2	5	0
G12	5	5	5	5

Q.2.9: Would you say there are any goals (research or other) that have particular significance in characterising:

i. the overall direction, and
ii. the general "feeling" (as you felt it) of the research over the period under consideration?

Are there any differences now?

APPENDIX 13 (cont.)

Q.2.10: Do you have (or did you have) any other goals (these could be political, economic or social in nature) that could be of importance to my understanding of the research in the period under consideration?

Q.2.11: How much sense do you think there is in dividing off certain research activities from others (as I have done in looking at the dopamine/octopamine program)? What other research were you conducting at the same time, and if you were involved with other research, how interconnected were your different projects?

APPENDIX 13 (cont.)

Letter:

"The University of Wollongong,
Department of Sociology.

Ref: TJ/MM

14th September, 1978.

Dear

As promised here's a revised version of the "flow diagram" that you saw and commented upon earlier this year. I've attempted to incorporate as many of the suggestions I received as possible, but if this version still does not adequately capture events as you remember them, please add further corrections where necessary.

Kind regards, and thanks for your co-operation,

Yours sincerely,

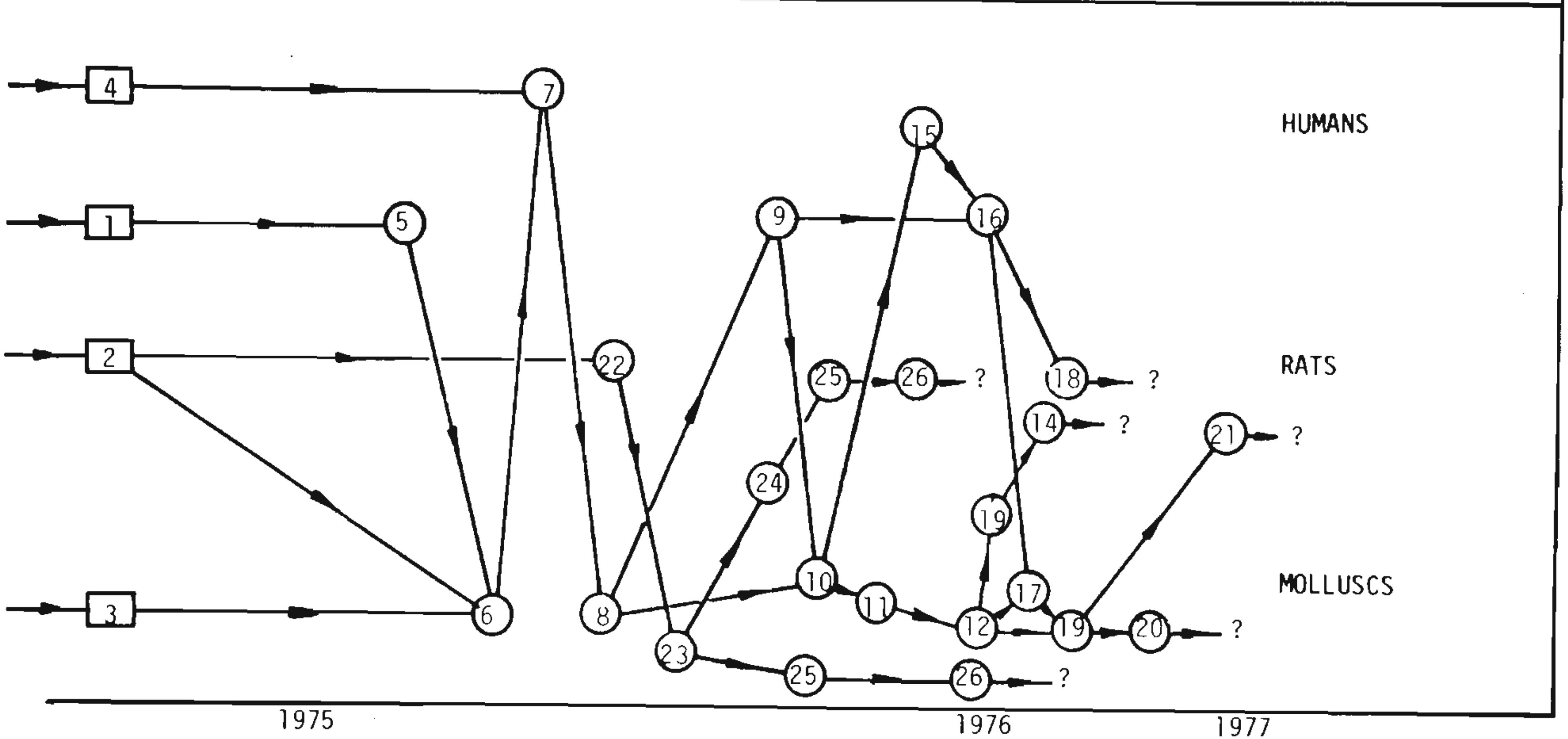
Tom Jagtenberg,
Department of Sociology,
UNIVERSITY OF WOLLONGONG."

Encl.

APPENDIX 13 (cont.)

FIGURE 3: Flow chart of significant research events in the evolution of the dopamine/octopamine program (Round 2 synthesis).

Approximate entry point of goals:	1 9a* 5*	5 11a	2 7 9a 11b	3 4 8 9b & c	6 10 12	Biological system focus



* These goals had a very different context before the establishment of the dopamine/octopamine program.

APPENDIX 13 (cont.) [Figure 3 cont.]

KEY TO FIGURE 3: Significant research events in the evolution of the dopamine/octopamine program.

Note: For greater ease of comprehension this list has been arranged to demonstrate something of a logical development of events. This reconstructed logic is sometimes at the expense of the chronological sequence of events.

LINES OF RESEARCH:

1. Clinical studies of Parkinson's Disease.

2. Drug absorption and protein binding.

3. Dopamine receptors in molluscan tissue.

4. --------'s research.

PARTICULAR EVENTS:

5. Adverse side effects (psychosis-like) were observed after the administration of l-dopa to a patient with Parkinson's Disease - it appeared as if the normal l-dopa effect was being blocked somehow. The patient had also been given the gastric emptier metoclopramide.

6. Metoclopramide demonstrated to be a dopamine antagonist.

7. It was hypothesised that dopamine antagonists might be derived from dopamine itself and produce the "on-off" effect in Parkinson's Disease.

8. Work on dopamine metabolites in molluscan tissue.

9. The existing dopamine hypothesis of schizophrenia began to be questioned and reformed. It was hypothesised that schizophrenia might be due to a failure to produce a dopamine antagonist.

10. It was hypothesised that there are multiple sites for the reception of dopamine. Attempts were made to sub-divide dopamine receptors in the mollusc and to test for blocking by anti-psychotic drugs.

11. Multiple sites found for the reception of dopamine in molluscs.

12. The anti-psychotic drug Clozapine does not act as a dopamine blocker on one particular dopamine receptor size.

APPENDIX 13 (cont.) [Figure 3 cont.]

13. An investigation for multiple sites for dopamine reception in rat brain is desired.

14. Conceptual and technical difficulties (such as the lack of suitable dopamine agonists) halted this line of research.

15. It was hypothesised that there are multiple sites for the reception of dopamine.

16. The dopamine/octopamine hypothesis: it was hypothesised that anti-psychotic drugs might block octopamine or related phenolamine metabolites of dopamine, and that this blockade might be responsible for anti-psychotic effects. (This followed from the observation that anti-psychotic drugs didn't appear to block any specific dopamine receptor, that is to say, there didn't appear to be any one specific receptor associated with anti-psychotic effects).

17. Clozapine demonstrated to block octopamine. (This work was stimulated by a comparison of the anti-psychotic effects of Clozapine and Perlopen reported in the literature.

18. Rats adopted defensive posturing when given octopamine or octopamine precursors (I.V.). (This was taken as supporting evidence for the importance of octopamine in psychosis/ schizophrenia).

19. Specific receptor sites for octopamine found in molluscan neural tissue.

20. Data collected to support an octopaminergic neuronal pathway in molluscs.

21. Indirect evidence of two specific dopamine sensitive receptor sites in rat tissue was received from overseas - "it was then that we realised we weren't alone".

22. Work on the uptake and release of dopamine from rat brain.

23. Octopamine found in large amounts in the neural tissue of molluscs.

24. Major technical problems experienced with the working up of octopamine assays to detect small amounts of the amine.

25. Major review and sytematisation of the present status of research undertaken.

26. Development of chemical assays based on mass spectrometric techniques. This entailed the synthesis of deuterated dopamine metabolites.

APPENDIX 14: DOP social networks questionnaire.

Letter: "The University of Wollongong, Department of Sociology.

Ref: TJ/MM

28th September, 1978.

Dear

I'm interested in obtaining a somewhat more detailed idea of the social networks relevant to the dopamine/octopamine program over the period January, 1975 to October, 1976 (approximately) - you may recall that this period corresponds with the time span of the events detailed on the various "flow charts" that you've commented on.

Towards this end I would appreciate your assistance in drawing up a list of the people with whom you have had interaction of some relevance to your research over the period under consideration. I'm interested in a fairly wide range of types of relationship and consequently it would help if you could classify these types of relationships using the following code:

Possible type of relationship with you	Code
co-worker	a
regular colleague	b
co-author	c
advisor	d
supervisor	e
student of yours	f
technical assistance to you	g
occasional colleague	h
occasional discussions	i
communication through the literature	j
elite peer	k
representative of funding organisation	l
any other(s) of importance?	m

.../2.

APPENDIX 14 (cont.)

2.

In addition I need to "locate" these people whom you list -
(a) in some organisation(s), and
(b) with respect to their major scientific interests.

Towards this last end I have listed below the components of the "theoretical landscape" that you've seen before.

Scientific interests of people with whom you've had any of the above types of relationships	Code
Clinical pharmacology	T1
Biology	T2
Medicine	T3
Biochemistry	T4
Neuropharmacology	T5
Neurophysiology	T6
Neural transmission theory, particularly the role of catecholamines and phenolamines as neurotransmitters in the nervous system of:	
(a) invertebrates (viz: mulluscs) (b) vertebrates (viz: rats) (c) man	T7
Dopamine theory of schizophrenia	T8
Dopamine/octopamine theory of schizophrenia	T9
Noradrenalin theory of schizophrenia	T10
Serotonin theory of schizophrenia	T11
Any others relevant?	T12

Regards,

Tom Jagtenberg,
Department of Sociology,
THE UNIVERSITY OF WOLLONGONG."

APPENDIX 14 (cont.)

SOCIAL NETWORKS QUESTION.

I've started this list with a hypothetical example to make things clearer:

Person	Organisational location	Type(s) of relationship with you	Major scientific interests of person
Linus Pauling	Wollongong Uni.	g, i, k.	T7, T8.

APPENDIX 15: Levels of achievement of the goals of the DOP and rates of progress towards the goals.

GOAL	Level of achievement of goals (0-10 scale)					
	April 1975		October 1976		February 1978	
	Level	Mean deviation (M.D.)[4]	Level	M.D.[4]	Level	M.D.[4]
1	4.0	2.0	4.0	2.0	6	
2.1	5.0	3.0	5.0	3.0	8	
2.2	4.0	2.0	5.0	3.0	8	
3	(3.0)[2]		3.0	1.0	4	
4	(2.0)		3.0	1.0	4	
5	4.0	2.0	3.0	1.0	6	
6	(3.0)		4.0	2.0	6	
7	4.0	2.0	5.0	3.0	8	
8			3.0	1.0	4	
9	3.0	1.0	5.0	3.0	8	
10.1	3.0	1.0	5.0	5.0	8	
10.2			5.0	3.0	8	
10.3			2.0	0	4	
11.1			10.0	0	10	
11.2			4.0	2.0	8.0	2.0
11.3			4.0	2.0	6	
11.4			6.0	4.0	6	
11.5			4.0	2.0	7	
11.6			4.0	2.0	8	
12.1			8.0	2.0	10	
12.2			4.0	2.0	10.0	0
13			0	0	10	
14[3]						
15[3]						
16[3]						

APPENDIX 15 (cont.)

GOAL	Level of achievement of goals July 1978		Rate of progress April 1975-Feb. 1978 (units/month)[1]	Average rate of progress $\bar{R}$ (units/month)
	Level	M.D.[4]		
1	6.0	0	6×10^{-2}	
2.1	8.0	0	9	
2.2	7.0	1.0	11	
3	5.0	1.0	3	$\bar{R}_{1-6} = 7 \times 10^{-2}$
4	3.0	1.0	3	
5	6.0	0	6	
6	6.0	1.0	9	
7	9.0	0	11	
8	4.0	0	11	
9	8.0	0	14	
10.1	10.0	0	14	
10.2	7.0	1.0	23	
10.3	4.0	0	11	
11.1	10.0	0	29	$\bar{R}_{7-13} = 19 \times 10^{-2}$
11.2	9.0	0.5	23	
11.3	7.0	1.0	17	
11.4	5.0	1.0	17	
11.5	7.0	1.0	20	
11.6	7.0	1.0	23	
12.1	10.0	1.0	29	
12.2	10.0	0	29	
13	10.0	2.0	29	
14^{+}	6			
15^{+}	4			
16^{+}	2			

APPENDIX 15 (cont.)

NOTES:

1. These rates have been calculated, for the sake of consistency with the rates calculated for the SSP, over a period which extends 16 months beyond the termination of events on the DOP flow diagrams. Also for the sake of comparison, it has been assumed that the levels of achievement for goals that entered consideration after April, 1975 were 0 in April 1975.

   Note: Average levels of achievement ($\overline{AT}$) of higher level goals and more technical goals at the cut-off points of the in-depth analysis (January 1977), using an estimating procedure based on the October 1977 figures and the average rates of achievement are:

   $\overline{AT}_{1-6}$ (Jan., 1977) = 4.6

   $\overline{AT}_{7-13}$ (Jan., 1977) = 5.2.

2. These bracketted figures are based on estimates for July, 1975.

3. These new goals (as described by the program leader) were:

   G14: To study the metabolism of phenolamines in the molluscan brain.

   G15: To study the metabolism of phenolamines in the mammalian brain.

   G16: To relate the group's work on multiple dopamine receptors to that of other workers with similar findings.

4. The mean deviations derive from the first round responses.

APPENDIX 16: Goal orientation of the publications of the members of the DOP in 1975-1977.+

Goal	Number of publications directed towards particular goal *	Number of authors involved	
		Program members	Non-members
1	0	-	-
2.1	0	-	-
2.2	2	3	1
3	0	-	-
4	2	3	1
5	0	-	-
6	3	3	0
7	0	-	-
8	5	5	1
9	0	-	-
10.1	10	3	0
10.2	3	4	2
10.3	0	-	-
11.1	0	-	-
11.2	0	-	-
11.3	0	-	-
11.4	0	-	-
11.5	1	3	1
11.6	1	3	0
12.1	0	-	-
12.2	0	-	-
13	0	-	-

* The sum of the numbers in the first column (27) does not equal the total number of publications of the group, since individual publications were often felt to be directed towards more than one goal. The level of goal specificity of the publications is listed in Appendix 17, which follows.

APPENDIX 16 (cont.)

\+ Program members were asked to match a list of the group's publications against the list of research goals on the basis of goals that were highly influential in accounting for the orientation of the publication. Because there were differences of opinion I have included all the papers indicated by the individual scientists. These numbers are then maximum numbers - the numbers of papers listed by individual was invariably smaller. This difference does not detract from the interpretation given in the text, however.

("Publication" here means refereed journal articles or conference presentations).

APPENDIX 17: Specificity of the DOP's publication record in the period 1975-1977.

Possible number of different goals towards which a publication was said to be directed	Numbers of publications directed towards particular numbers of goals
1	16
2	3
3	1
4	0
5	0
6	0
7	0
8	0

Total number of publications in the period 1975-1977 * = 20.

* Some of the publications listed here had been accepted for publication in the period 1975-1978, but were not actually published until 1978.

APPENDIX 18: Priorities and autonomy indices for the DOP.

Goal	Average priority for core group (0-10 scale)[1]	Mean deviation (M.D.)[2]	Autonomy index for core group	M.D.[2]
1	0	0	0.5	3.5
2.1	0	0	0	3.0
2.2	0	0	0	3.0
3	5	5.0	1	4.0
4	4.5	4.5	0	3.0
5	4.0	4.0	-0.3	0.5
6	5.7	3.7	1.5	1.5
7	7.4	0.4	1.5	0.5
8	3	3.0	1.5	0.5
9	2.5	2.5	2.0	2.0
10.1	3.0	3.0	1.0	1.0
10.2	3.0	3.0	1.3	1.2
10.3	0	0	2.3	1.7
11.1	4.2	0.3	0.3	0.3
11.2	1.4	1.4	0.5	1.0
11.3	2.0	1.0	2.0	0
11.4	0	0	1.0	2.0
11.5	0.5	0.5	1.5	1.5
11.6	0	0	0.5	1.5
12.1	0	0	-0.5	0.5
12.2	0	0	-0.5	0.5
13	0	0	3.0	1.0

1. These figures have been re-scaled from a rank order scale to a 0 (low) to 10 (high) scale.

2. The mean deviation derives from the first round responses.

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